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FOLK-LORE OF THE BAHAMA NEGROES.

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The Bahamas include over 3,000 islands, but most of these are small, grouped about a few larger islands and these groups are not separated by great distances, so that as a whole the Bahamas present a striking homogeneity, both in their origin as coral formations, and in the life and surroundings of the people. As the biologist of to-day gains a better insight into the complex problems of structure and relation presented by living things, by mastering the life history of one or a few species, so, in the study of the folk-lore of a people, can we gain a better knowledge of its philosophy by considering the physical environment and the history of some particular community of that people.

The material for this paper I gathered during the summer of 1888 at Green Turtle Cay, one of the "out islands" of the Bahamas, which because of its isolation presents the simplest conditions in the life of the people and has all of the physical features peculiar to the Bahamas. One of the broken parts of an ancient fringing reef to the island of Great Abaco, of only a few square miles in area, this island, as its name (from the Spanish *cayo*) indicates,

is only *a rock* in the ocean. Like a hundred others in the same series it appears from a distance as a dark low-lying sand-bar, but when closely approached is seen to be covered, for the most part, with short trees and bushes, while occasionally a clump of tall cocoa-palms is sharply outlined against the bright sky. The vegetation is of as deep a green as the sea is blue beyond the present coral reef, while between the vegetation and the water the triturated skeletons of corals and echinoderms and the shells of molluscs, constituting an intensely white coralline sand, glare in the sub-tropical sun. The shoals about are also of this constantly shifting sand, and so the shallow water is rendered a chalky green shade, beautifully contrasting with the wonderful blue of the deeper sea.[1]

The island of Great Abaco, or in native vernacular, "the main," lies about three miles away to the South-west, while to the North and East, unbroken but by the top of the present coral reef, marked at each wave by a white line of spray, extends the vast Atlantic.

It is on the south-west side where the wide channel between the Cay and "the main" affords such an excellent roadstead for vessels, that the town has been built. In this channel, the swell of the ocean, so much broken by the outer reef and the line of cays, is perceptible only in the small passages between cay and cay, and to a less extent along the shore of Great Abaco, while for the most part this water is as smooth as a lake, disturbed only by the tides and winds. It is a sight of peculiar beauty to see on a summer morning the small boats of the "Conchs," as the natives both white and black are called, scudding over the blue waters of

[1] These shoals sometimes reach gigantic proportions, as in the Great Bahama Banks where over hundreds of square miles the water is but a few fathoms in depth.

the channel to points on "the main" for ten miles on either hand. They go to the "pine fields;" for the raising of pine-apples is the principal farming industry at Green Turtle Cay, and most of the men who cultivate the plants live in the town on the cay, sailing to their farms in the morning and back at night.

There is practically no soil as nature leaves the land, and so the plants found there are such as can adapt themselves at the root to crannies in the rock and there gain some sustenance from the mould of their ancestors, while from the air, the leaves may breathe in the rich supplies of gasses and moisture, always emanating under the sub-tropical sun. So farming implements in the Bahamas, as has been aptly and almost truthfully said, are the pick-ax and *machete*. With the former a natural crevice in the rock is somewhat widened, and therein a pine-slip or some seed is planted, while with the *machete*, a long, broad-sword sort of knife, the weeds and bushes are cut down.

The town of Green Turtle Cay was founded by a family of loyalist refugees who fled from the American colonies during the revolution. It has grown slowly, for the most part by the natural increase of the few first families; and, because of repeated intermarrying among these family stocks, at present nearly all of the people are inter-related. The population, about evenly divided on the basis of color, is of some fifteen hundred souls.

If one will imagine a sea-coast town in North Carolina, (as much as possible isolated from railroads and ocean steamers, and its people leading a seafaring life with farm work at odd intervals,) transported to a small coral island, then one can gain a very fair outline for the picture of Green Turtle Cay. But there are touches of local coloring quite necessary to complete the picture.

There being no horses and carriages on the cay, roads for their accommodation are not essential, and so the streets are not wider than a city sidewalk, and the squares, into which the town is divided, are proportional to the streets in size. The streets are formed by smoothing off the naturally jagged points left by the action of water upon the coral sandstone of which the cay is composed, and they are of dazzling whiteness. The houses are generally of frame, three or four sometimes crowded upon the same small lot, and, whenever the owner can afford the display, painted white, a most disagreeable reflection of the glare from the street and sea-shore.

The people are intensely pious. The whole social life centers in the church. Those mad days of the Buccaneers and their nominally more respectable descendants, the Wreckers, are gone. For the ribald song of the riotous pirates, we have the solemn hymns of the Wesleyans, and the chant of the English church. Light-houses have taken from the coral reefs their former terror. But after some residence among the people, one is compelled to suspect a shallowness in their piety, a great deal of selfishness in their character. Their conceit and petty social distinctions are such as all isolated provincials exhibit.

The laws against swearing are quite severe and, what is even more necessary, the good old patriarch who holds all of the offices from chief magistrate to street commissioner, is quite strict in the enforcement of the laws, so that the ordinary street talk is quite a relief to one who is familiar with the profanity of American streets.

The colored people, everywhere gossipy, good-natured and religious, having here been emancipated for over fifty years, have become somewhat educated and unusually independent. Socially the races are more nearly equal than anywhere else on the globe.

Schools and churches are occupied in common. Miscegenation, so prevalent in Nassau, the capital, has not prevailed in this colony to any extent. The first few negroes who came to the cay were slaves of the loyalists, and then from a shipwreck about 1837 a few more from America were stranded in this colony, but aside from these, the large majority indeed, have come as direct descent from native Africans, and there yet lives one old negro "Unc' Yawk," who, bowing his grizzled head will tell you: "Yah, I wa' fum Haf'ca."

It is with the negroes that one associates the picturesque and beautiful surroundings in the Bahamas. Their huts, so often thatched with palmettos, are built on the low, sandy soil of the town. There grow the graceful cocoa-palms with long green leaves whose leaflets rustle as sadly as do those of oak and chestnut in the autumn woods of the north. There, too, the prickly pear, like an *abatis*, bristling all over with needles, seems to guard the luxuriant blossoms of the great oleander bush, dispensing sweetest perfumes from its midst. Apparantly every hut has its quota of a dozen little black "Conchs" of assorted sizes, who think it a palace beneath the palmetto roof and the yard a menagerie wherein the pigs, and chickens, and dogs are animals worthy of study.

There are no chimneys in the Bahamas and but few stoves. Boiling and frying are done in a small shed over an open fire, built on a box of sand; while for the baking is employed an oven of the same sort as our foremothers knew by the name of the "brick oven." It is a cone made of coral sand-stone into the upper half of which is hollowed an oven. In this oven a fire is built and kept burning for several hours until the rock is quite hot, then the fire is raked out and the food to be baked is placed in the oven. The "mammy" and children do most of the

work while the lord and master plays checkers or lies in a hammock reading a novel.

There is one piece of work however in which man and wife share, and that is the chastisement of the children. They chastise with a club, and regularly every twenty-four hours the screaming of the tortured child comes from the hut or surrounding bushes, to tell its sad tale of remaining barbarism; but the negro child has a disposition full of sunshine, and in a few moments after being beaten, will sing like the happiest being on earth.

In the evening is the play time of the negroes. The children gather in some clump of bushes or on the sea-shore and sing their songs, the young men form a group for a dance in some hut and the old people gossip. The dance is full of uncultured grace; and to the barbaric music of a clarionet, accompanied by tambourines and triangles, some expert dancer "steps off" his specialty in a challenging way, while various individuals in the crowd keep time with beating of feet upon the rough floor, and slapping of hands against their legs. All applaud as the dancer finishes; but before he fairly reaches a place in the circle, a rival catches step to the music and all eyes are again turned toward the centre of attraction. Thus goes the dance into the night.

The strangest of all their customs is the service of song held on the night when some friend is supposed to be dying. If the patient does not die they come again the next night and between the disease and the hymns, the poor negro is pretty sure to succumb. The singers, men, women and children of all ages, sit about on the floor of the larger room of the hut and stand outside at the doors and windows, while the invalid lies upon the floor (which Bahamans generally, both white and black, prefer to the bed) in the smaller room. Long into the night they sing their most mournful hymns and

dirges and only in the light of dawn do those who are left as chief mourners silently disperse. The following dirge is the most often repeated, and with all their sad intonation accented by tense emotion, it sounds in the distance as though it might well be the death triumph of some old African chief:

mf
If hev - ry day vwas judg - ment day
p
Some - bod - y's dy - ing here to - day.
mf
O John - ny, fur true you' called to - day,
p
pp
Some - bod - y's dy - ing here to - day!
REFRAIN.
ff
Hev - ry day pass - ing a - vay,
Hev - ry day pass - ing a - vay
ff
mf
Hev - ry day pass - ing a - vay,
p
pp
Some - bod - y's dy - ing hev - ry day!

Each one of the dusky group, as if by intuition, takes some part in harmony, and the blending of all pitches in the soprano, tenor, alto and base makes such peculiarly touching music as I have never heard elsewhere. As this song of consolation accompanies the sighs of the dying one, it seems to be taken up by the mournful rustle of the palms and lost only in the undertone of murmur from the distant coral reef. It is all weird and intensely sad.

The folk-tales are most popular among the children, and, indeed, are no doubt handed down from generation to generation principally by them. After the short twilight and the earlier part of the evening, when singing and dancing amuse the children, comes the story-telling time *par excellence*. This is usually about bed-time and the little "Conchs" lie about upon the hard floor of the small hut and listen to one of the group, probably the eldest, "talk old stories." With eyes that show the whites in exclamation, and ejaculations of "O Lawd!" "Go!" "Do now!" etc., long drawn out in pleasure, the younger ones nestle close together so "De Debble" wont get them as he does "B' Bookey" or "B' Rabby" of the story.

These tales are divided into two classes—"old stories" and fairy stories—the former particularly constituting the negro folk-lore, while the latter have been introduced from the same source as the ordinary fairy-tales of English children. It is a curious fact that some of the fairy-tales have been translated, so to speak, into old stories, and one easily recognizes in such a tale as "B' Jack an' de Snake" its English ancestor of "Jack the Giant-Killer."

The folk-lore proper is mostly concerning animals which, personified, have peculiar and ofttimes thrilling adventures. Where, in our own negro-lore,

the animals are called "Brer," among the Bahama negroes this term is contracted to "B," and so one finds in "B' Rabby, who was a tricky fellow," the "Brer Rabby" whom Uncle Remus has made famous to us as the hero of the folk-lore of the South.

The conventional negro dialect, used in our American stories, will apply to the Bahama negroes only in part, for their speech is a mixture of negro dialect, "Conch cockney," and correct English pronunciations. In the following stories, which are given as nearly as possible verbatim, this apparent inconsistency will be noticed, for in the same story such expressions, for example, as "All right' and "Never mind," may be given in the cockney, "Hall right," in negro dialect, "Ne'r min'," or pronounced as written in correct English, and one may never know which pronunciation to expect.

In these stories one readily detects the influence of physical environment and the play of native invention, in the predominance given to those animals and plants locally prominent, acting their parts among scenes borrowed from local surroundings. On the other hand, the introduction of the lion, elephant, and tiger suggests an heredity from African ancestors, while similarly the rabbit, in the title rôle of hero, as rabbits are not indigenous in the Bahamas, points to the influence of American negro-lore. The isolation of the Bahamas from foreign influences, the scanty supply of books and newspapers, and the great lack of what are generally termed amusements, has given especially good conditions for the development of a folk-lore at once recognized as peculiar and sectional.

An atmosphere, that paper cannot hold, is added to these tales by the physical conditions;—an island out in the Atlantic, arising with low shores from that indescribable blue water, covered by the paler blue

of the skies of "Summerland," heated by the glaring sun of mid-day, or bathed in silver radiance by the queen of night; the querulous gulls, catching fish in the shallows or "white water;" the cunning little lizards which, from orange tree and stone wall, watch your every step; the dazzling white of the streets, and the superficial piety of the people; the sea gardens, where, in the clear water, one beholds the fans and feathers of the sea waving in response to tide and billow; beneath them the creeping stars, the spiny urchins and long cucumbers crawling among the tentacled anelids and anemones, and chasing in and out, above and around these more simply organized creatures, the fishes, banded in gold and black and orange, with long, waving filaments to their fins and high foreheads, which solemnly suggest an intellect only developed in higher forms; and then, finally, those colonies of coral animals, which inhabit the top of a submarine precipice built of the skeletons of their ancestors through millions of generations, and which ere long will die to complete the foundation for another island or series of islands.

There is perpetual beauty on land and in the sea, while the balmy, even-tempered air invites one to sail over the blue waters or to lie in a hammock beneath the palms and listen to some black "Conch" "talk old stories." One boy named Dennis was very much the best story-teller on the cay, and from him I took most of the following tales, but the quick, short gesture, the peculiar emphasis on the exciting words and phrases, the mirth now bubbling from eyes which anon would roll their whites in horror, in short, the *Othello* part of the tales, I cannot give.

II.[1]

B' Rabby in de Corn-field.

Once 'twas a time, a very good time,
De monkey chewed tobacco, an' 'e spit white lime.[2]

So dis day, it vwas a man; had a big fiel'; peas, corn and potato. De man didn't used to go in de fiel.' 'E send his boy. So dis day B' Rabby come vw'ere de boy vwas. 'E say, "Boy, you' pa say, gi' me some peas, corn and potato." 'E let 'im eat as much as 'e vwant. De nex' day B' Rabby come back again. 'E say, "Boy, you' pa say, gi' me some peas, corn an' potatoes." So now vw'en de boy vwent home de boy say, "Pa, you tell B' Rabby to say, I say must give 'im peas, corn an' potatoes?" De man say, "No, I aint see B' Rabby." 'E say, "De nex' time B' Rabby come dere you mus' tie B' Rabby an' let 'im eat as much peas, corn an' potatoes as 'e like." 'E say, "You mus'n' let 'im go." De nex' day B' Rabby come. 'E say, "Boy, you' pa say, 'Gi' me some peas, corn an' potatoes.'" B' Boy say, "Le' me tie you up first." B' Rabby say, "All right, but vw'en I done eatin' you mus' let me go." B' Boy say, "All right," too. Now vw'en B' Rabby vwas done eatin,' B' Rabby say, "Boy, le' me go now!" B' Boy say, "No!" B' Rabby say, "Min', you better le' me go!" B' Boy say, "No!" B' Boy vwent to call his pa. B' Boy say, "Come pa, got 'im to-day!" De man vwent over in de fiel'. Dey ketch B' Rabby. Bring 'im up; put 'im in de hiron cage. Now dey had on six big pots o' hot vwater. B' Tiger vwas

[1] As the purpose of this paper is more to be a record of fact than a literary production, I must again call attention to the fact that there is not an invariable dialect among the Bahama negroes, the same word often being pronounced differently in succeeding phrases or sentences. My effort therefore has been to report these tales phonetically as I heard them, and not upon themes given to write new stories.

[2] The old stories are almost always introduced by this doggerel verse, and very often some expression, as, "Twant my time: twant you' time; was old folks' time;" is added.

comin' past. 'E say, "Vw'ats de matter, B' Rabby?" 'E say, "Dey got me in here to marry de Queen's daughter, an' I don' vwan' to marry 'er." Now 'e say, "D' see dem six pots dere? Dey got dem full, *full* o' cow-'eads for my weddin'." B' Tiger say, "Put me in, I vwan' to marry de Queen's daughter." B' Rabby say, "Take me up!" B' Rabby jump up. 'E fasten up B' Tiger. Now B' Rabby *gone!* 'E git in one hollow, hollow pison-wood tree.

De boy come out an' say, "Pa, dey big one here!" De man say, "Don' care if 'e big one or little one, I goin' to scal' 'im. De man come out. 'E git de hot vwater. 'E take de big pot full, *boilin' up*; 'e swash on B' Tiger. B' Tiger, 'e holler, "*Tain' me! tain' me!*" De man say, "Don't care 'f tain,' you or vw'at, I goin' scal' you." 'E scal' B' Tiger. B' Tiger 'e give one jump; 'e knock de cage all to pieces. B' Tiger *gone!*

'E come to dis same pison-wood tree B' Rabby vwas in. 'E sit down right on de stump o' de tree vw'at B' Rabby vwas in. B' Rabby had one sharp, sharp stick, an' 'e shove right into B' Tiger. B' Tiger say, "*My goody!*" 'e say, "Hants here!" B' Rabby take de stick; 'e shove it out; 'e stick B' Tiger. B' Tiger say, "No dis aint hants; B' Rabby here." Den B' Tiger look down in de hole an' 'e see B' Rabby settin' dere. B' Tiger say, "*Ha-an!* B' Rabby! Never min', you cause me to get scalded;" 'e say, "N'er min'! I goin' ketch you!" B' Rabby say, "Move boy! Le' me git out! Doan' min' me!" B' Rabby *gone!*

B' Rabby see one dead goat in de road. De goat dead; stink and be rotten. All de goat back vwas rottin' avay. B' Rabby *gone;* 'e git inside de dead goat.

B' Tiger vwas comin' fas'! Vw'en B' Rabby look, 'e see B' Tiger comin'. B' Rabby vwas doin,

DO NOT

"*Huhn! huhn!*" vwas doin' so in de goat, "*Huhn! huhn!*" B' Tiger say, "Vw'ats de matter, B' Goat?" 'E say, "B' Rabby vwen' past here just now; poin' he finger at me an' rottin' avay all my back." B' Rabby *gone* out de dead goat.

B' Rabby vwas fishin.' 'E see B' Tiger. B' Rabby had he finger bent. B' Rabby say, "Min', I goin' poin'!" B' Tiger say, "*Do* B' Rabby, doan' poin'!" Dats vw'at B' Tiger say. B' Rabby didn' poin.'

[1] E bo ban, my story 's en',
If you doan' believe my story 's true
Hax my captain an' my crew.
Vw'en I die bury me in a pot o' candle grease.

B' Helephant and B' Vw'ale.

Once it vwas a time, etc.

Now dis day B' Rabby vwas walkin' 'long de shore. 'E see B' Vw'ale. 'E say, "B' Vw'ale!" B' Vw'ale say, "Hey!" B' Rabby say, "B' Vw'ale, I bet I could pull you on de shore!" B' Vw'ale say, "You cahnt!" B' Rabby say, "I bet you tree tousan' dollar!" B' Vw'ale say, "Hall right!" 'E *gone*.

'E meet B' Helephant. 'E say, "B' Helephant," 'e say, "I bet you I could pull you in de sea!" B' Helephant say, "Me!" 'E say, "Dey aint ary man in de worl' can pull *me* in de sea!" B' Rabby say, "I'll try it to-morrow at 12 o'clock."

'E gone an' get a heap o' rope. 'E say, "Now to-day we'll try it." 'E tie one rope aroun' B' Vw'ale's neck, and den 'e tie one aroun' B' Helephant's neck. 'E say, "Vw'en you hear me say, '*Set taut*,' you must set taut." 'E say, "*Pull avay!*" Vw'en B' Vw'ale pull, 'e pull B' Helephant in de surf o' de sea. 'E say,

[1] The first three lines of this doggerel verse form the customary ending of a story, while the last line may be added to suit the individual fancy of the narrator.

"You tink dis little B' Rabby doin' all o' dat!" W'en B' Helephant pull taut, 'e pull B' Vw'ale in de surf o' de sea. B' Vw'ale ketch underneath one shelf o' de rock, and B' Helephant ketch to one big tree. Den de two on 'em pull so heavy de rope broke.

B' Vw'ale went in de ocean and B' Helephant vwent vay over in de pine-yard. Das vy you see B' Vw'ale in de ocean to-day and das vy you see B' Helephant over in de pine bushes to-day.

'E bo ban, etc.

B' Rabby, B' Spider an' B' Booky.

Once it vwas a time, etc.

B' Rabby, B' Spider an' B' Booky wen' in de field. As evenin' vwas comin' dey was comin' home in de boat. An' dey had one bunch o' bananas to share an' dey didn' know how to share it. An' B' Spider did say to B' Rabby, "Trow de bunch o' bananas overboar' an' den who could *dive* de mostest could *have* de mostest."

Den dey pull off der close. B' Rabby had de furst dive. Vw'en 'e vwent down to bottom 'e bring up four bananas. Vw'en B' Booky vwent down 'e bring two. B' Spider vw'en 'e pitch overboar' 'e float.

B' Rabby pitch overboar' again an' 'e bring up six. B' Booky pitch overboar' again an' 'e bring up four. B' Spider pitch overboar' again; 'e *float*. 'E say, "You no tie de grapple to me an' le' me go down an' get hall." An' 'e vwent down; 'e ketch hall on 'em; an' 'e couldn' come hup no more.

An' B' Rabby take his knife an' cut avay de rope. An' den dey vwent home. An' B' Spider———; vw'en dey hax 'em, "Whey B' Spider?" An' B' Rabby say, "B' Spider 'e did have such a big eye;

'e did vwant all de bananas an' 'e couldn' dive." 'E say, "B' Spider did say, 'You no tie dis grapple to me an' le' me get hall.'" An' de Spider's mudder say, "'F you don' go fetch 'im I put you in prison." An' dey say dey aint gwine. An' de vwoman did carry 'em to prison. An' B' Rabby did put de vwoman in jail. An' de judge did say, 'e couldn' put B' Rabby in prison fur dat, cause 'twas B' Spider's fault.

E bo ban, my story 's end, etc.

B' Man, B' Rat an' B' Tiger-Cat.

Once it vwas a time, etc.

So now dis day; ebry time de rat use' to go in de man' field eatin' de man' peas, potatoes an' his corn. So now dis day de man ketch de rat; 'e had de rat in de cage to kill 'im. De rat say, "*Do*, B' Man, spare my life, I'll never come back any more!" De man say "*Hall* right!" 'E let de rat go. B' Rat vwent vay hover in de vwood; 'e never come back any more.

Dis day de man vwent shootin' pigeons. 'E vwent vay over in de vwoods; 'e shoot a big bunch o' pigeons. Now 'e *gone*, 'e see tree young tiger-cats. De man vw'en' 'e gone 'e take all tree de tiger-cats. Soon as 'e make one step de hold tiger-cat, 'e after 'im an' growl. De tiger-cat say, "Taint no good to put down my young ones so you might as well keep 'em. B' Tiger-cat say, "B' Man, le' me tell you vw'at to do."

B' Tiger-cat vwas hup in one tree ready to pitch on de man. B' Tiger-cat say, "You let de dog heat de pigeons; you heat de dog, an' let me heat you." De man stan' up an' 'e study. B' Tiger-cat say, "Talk fas', B' Man; talk fas'!" 'e say, "Let de dog heat de pigeons; you heat de dog, an' le' me heat

you." Den dat same rat w'at de man let go jump out de road. 'E say, "Yes, B' Man, do dat; give de pigeons to de dog; you heat de dog, an let B' Tiger-cat heat you, an' let me heat B' Tiger-cat."

B' Tiger-cat stan' up; 'e study. B' Man say, "Talk fas', B' Tiger-cat, talk fas!" B' Man vwent towards his gun. B' Tiger-cat jus vwas studerin' on vw'at B' Man say. Den de man pick up his gun. Vw'en 'e fire 'e shoot B' Tiger-cat dead. Den B' Rat jump up an' say, "One good turn deserve another! One good turn deserve another! One good turn deserve another!"

F bo ban, etc.

B' Booky an' B' Rabby.

Once it vwas a time, etc.

It vwas B' Rabby; 'e use' to go to de Queen's pasture ebry night an' take de bigges' sheep from de flock. So dis night vw'en' 'e wen', de Queen's servant did put a lion at de head o' all de huder sheeps. B' Rabby vwas takin' dat to be a sheep, an' 'e carried 'im a little vays in de road an' 'e say, "Look 'ere, dis t'ing don' vwalk like sheep, dis t'ing vwalk like lion!" An' 'e call out for B' Booky. 'E say, "B' Booky, 'ere, take dis sheep, I got to go up 'ere in de wood for de huder one I got tied in de fence!" An' vw'en B' Booky get a little vays, 'e sing out, 'e say, "Dis t'ing don' vwalk like sheep!" 'E sav, "Dis t'ing vwalk like lion!" Den 'e hollered out so his vwife an' children, tell dem all to get up in de roof o' de house. 'E holler out, say "De lion comin' to tear you to pieces!" An' vw'en de lion get to de house, 'e walk in tr'u' de door an' 'e see all on 'em up in de roof an' 'e look up at 'em.

An' de smalles' chil' say, "Fadder an' mudder, I

know you love me, but I cahnt hold out no longer!" An' de fadder say, "See lion 'ere!" An' vw'en de chil' drop de lion tear her to pieces. De huder one say, "Fadder an' mudder, I know you love me, but my harms is tired!" An' his fadder say, "See lion dere!" An' vw'en 'e drop de lion tear 'im to pieces. Dis de bigges' one now; 'e say, "Fadder an' mudder, I know you love me an' I love you, but I cahnt hold out no longer!" An' de fadder say, "See lion dere!" An' vw'en 'e drop de lion tear 'im to pieces. His vwife say, "I know you love me," an' she say, "I love you too!" De husband was so pitiful 'e couldn' talk, an' 'e jus' pint his finger down to de lion an' his vwife drop. An' after 'e see all on 'em vwas gone 'e vwent out tr'u' de roof o' de house an' 'e stay dere until de mornin', an' dats how 'e vwas save. B' Lion couldn' jump an' 'e vwen' avay.

E bo ban, my story 's end, etc.

B' Baracouti[1] an' B' Man.

Once it vwas a time, etc.

Once it vwas a man; 'e had a fiel' 'pon a differ'n' part o' de shore. Dis day 'e did vwan' to go to his fiel'; 'e met a shark. 'E said to de shark, "Please carry me 'cross to my fiel'!" B' Shark say, "All right!" an' 'e carried him 'cross. Vw'en 'e got 'cross 'e give B' Shark a good cut. B' Shark say, "*All* right!"

'E come out again from his fiel'; 'e meet B' Shark again. 'E say, "B' Shark, *please* carry me 'cross, once more!" B' Shark say, "All right!" An' B' Shark carried him 'cross again. An' 'e give B' Shark a heavy cut again. B' Shark say, "*All* right!"

[1] The Baracouti is an eel-like fish, with numerous, strong, sharp teeth, and is very savage when attacked.

De nex' day de man did vwan' go 'cross again. 'E say, "B' Shark, *please* carry me 'cross to dat shore;" 'e say, "I'll give you a fortune!" B' Shark carry 'im again, an' 'e give B' Shark *such* a cut, till B' Shark had to lay awake till 'e come out again.

Sun vwas nearly down vw'en de man come out. 'E say, B' Shark, *please* carry me 'cross again;" 'e say, "I'll pay you vw'en I get 'cross." B' Shark say, "Get on my back." De firs' fish B' Shark meet vwas a Corb.[1] B' Shark say, "B' Corb, you do man good an' man do you harm;" 'e say, "vw'at you mus' do to'r' him?" B' Corb say, "Cut 'im in two!" Nex' vwas a Porpy.[2] 'E say, "B' Porpy, you do man good an' man do you harm, vw'at you mus' do to'r' 'im?" B' Porpy say, "Leave it to God!" De nex' vwas a Baracouti. 'E say, "B' Baracouti, you do man good an' man do you harm, vw'at you mus' do to'r' 'im?" B' Baracouti say, "Cut 'im to hell!"

B' Shark see B' Rabby on de rocks. 'E say, "B' Rabby, you do man good an' man do you harm, vw'at you mus' do to'r' him?" B' Rabby say, "Come in little further, I ain' hear you!" (B' Rabby vwan' to save de man.) 'E come in. 'E say, "B' Rabby, you do man good an' man do you harm, vw'at you mus' do to'r' 'im?" B' Rabby say, "Come in little bit further; still I ain' hear you!" B' Shark come in a little bit further. 'E say, "I cahn' come no further else I get 'shore!" B' Shark say, "You do man good an' man do you harm, vw'at you mus' do to'r' 'im?" B' Rabby say, "Vy, let' im jump 'shore!" Before de Shark could turn 'round to go with 'im de man jump 'shore an 'B' Shark commence to cry.

E bo bau, my story 's en', etc.

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[1] Flat-headed shark, particularly dreaded as a man-eater.
[2] Porpoise.

B' Loggerhead an B' Conch.

Once it vwas a time, etc.

Dey vwanted de King's daughter. King told de two to have a race, de one dat beat de race to have his daughter. Dey hask him, "Vw'at sort o' race dey mus' have." 'E said 'e wanted to see who could vwalk de fastes' out o' two. Dat vwas de Loggerhead and de Conch. De Conch knowed dat de Loggerhead could beat 'im walkin' 'so de Conch vwent an' hired hother Conchs an' put 'em to de marks' stake. Den after dat 'e vwent down to de river whe' de Loggerhead vwas en' told 'im 'e's all ready for de race.

'Im an' de Loggerhead started off together. De first mark de Loggerhead get to 'e meet a Conch dere, takin' it to be de one dat 'e start off to race with, but standin' talkin'. De one dat went to race, 'e went ahead o' de Loggerhead. Den de Loggerhead started from the place where de Conch vwas, expectin' it vwas de same Conch. Vw'en 'e git to de nex' pole 'e meet a Conch again still thinkin' it vwas de same Conch. Stand dere dey small-talk; whilst talkin' give de Conch vw'at hired de other Conchs a chance to chat with 'im, den de Conch had chance to go 'is vway. Vw'en de Loggerhead git to King's palace, 'e met de Conch 'head of 'im. De Conch had beaten de race an' 'e got de King's daughter. Den after dat de Loggerhead say 'e vwould take de sea for 'is dwellin' place.

E bo ban, my story 's en', etc.

B' Crane-Crow, B' Parrot an' B' Snake.

Once it vwas a time, etc.

It vwas a heagle, layin' in a tree. Hafter she had young ones dis snake use to plague de tree. So

afterwards B' Heagle lef' B' Crane-crow an' B' Parrot to watch dese young ones, vw'en B' Snake come, to call 'er. So vw'en de snake come, dey call dis heagle Dey say, "Ma hoo heagle! De snake comin'!" So she come. Therefore she kill de snake. She said, "Hafter he het my young ones;" she say, "Therfore I'll go nord, I'll live dere all my life on de nordern part of Baltimore.[1] I no more to say. De snake had het my young ones." Dats makes so you see heagle live hover dere to-day; dey won' come dis side.

B' Cricket and B' Helephant.

Once it vwas a time, etc.

So it vwas palm-ile tree whe' dey use' to go to feed. So hevery time B' Helephant use' to go dere, 'e use to meet B' Cricket. 'E say, "B' Cricket, I bet I can mash you up some o' dese days." So B' Cricket say, "B' Helephant, you cahn' mash me up, fur it don' stan' fur de bigness o' man; little man could make big man run." B' Helephant say, "Go vay, B' Cricket!"

So dis day vw'en B' Helephant come to de tree B' Cricket vwas dere. B' Helephant didn' see 'im. 'E vwen' in B' Helephan' ear-'ole an' e' git to sing-in,' an' B' Helephant 'e did put off a runnin'. Every-whe's 'e put 'is foot it vwas river. So 'e meet B' Lion. 'E say, "B' Lion, man' mo'n me to-day;" 'e say, "I 'bout de bigges' beast in de fores' an' you 'bout de stronges'; an' still, if you vwas to hear vw'at I hear to-day, make you run."

"Let 'im come, I'm a man fur anything," dats de

[1] To the people of Green Turtle Cay, Baltimore, from whence come the schooners in the pine-apple trade, is one of the great places beyond the sea of which the children, especially, have peculiarly vague ideas.

word B' Lion say. B' Cricket jump out B' Helephan' ear-'ole an' gone in B' Lion ear-'ole, an' vw'en B' Cricket sing out in B' Lion ear-'ole, 'im an' B' Helephan' start together. B' Helephan' see B' Lion runnin', taught de soun' o' de cricket vwas still in 'is ear-'ole. Some iron-wood tree dere vwas six times big as dis house. B' Lion tear 'em right square up by de root.

Vw'en dey get dere dey meet B' Jack standin' on de hill. Jack say, "Vw'at you no runnin' 'ere 'bout?" Dey say, "B' Jack, man 'ere to-day mo'n you an' me an' you two together!" B' Jack say, "I'm de man to heat you an' de man too!" So B' Cricket jump out de lion ear-'ole an' vwen' in B' Jack own to tell 'im de hargemen' B' Helephan' an' 'im had under de palm-ile tree. So B' Helephan' 'e quiver so much 'e drop down dead. B' Jack say, "My deah man, dat vwas de harge vw'at you an' B' Cricket had." B' Cricket say, "I tell you 'bout a little man every day;" so a puff o' win' come an' end dis story.

E bo ban, etc.

B' Jack an' B' Snake.[1]

Once it vwas a time, etc.

De Queen say, "B' Jack, if you can kill dis snake, I don' know how much money I wouldn' give you!" So Jack say, "I wan' five hundr' dollars to go on a spree." So 'e gone up dere now. 'E say, "B' Snake, vw'at you tink dese foolish people say; dey say you' body cahn' go in dis half-hitch." 'E come out de hole an' 'e vwen' in de half-hitch. Den Jack draw de half-hitch taut. Den all dese soljers come around; dey cut 'im up in pieces.

[1] Probably founded upon "Jack the Giant-Killer."

So den de Queen say, "Jack, I got one more trial fur you to do." 'E say, "Vwell, vw'at dat is?" "If you could go up 'ere in dis corn fiel' an' kill all de rice-bird, I let you git married to my daughter," dats vw'at the Queen say. 'E vwen' to de fiel' to de rice-bird. So B' Jack say, "B' Rice-birds, vw'at you no tink dese foolish people say; dey say all o' you no rice-birds cahn' full up dis basket!" De rice-birds say, "Vy people so foolish, no all us rice-birds cahn' full up dat basket!"

So all de rice-bird vwent in de basket. So B' Jack drawed de basket together with de rice-birds in it. So B' Jack vwen' home to de Queen with dese rice-birds. Say "Her' de rice-birds." Queen say, "Vwell, B' Jack, you can get married to my daughter."

E bo ban, etc.

B' Crane-Crow an' B' Man.

Once it vwas a time, etc.

Now dis day the Queen did vwant a man to see if 'e couldn' ketch dis Queen Crane-Crow. De man gone whey all de Crane-Crows use' to come. Now de man lay down an' make believe 'e vwas dead. Now hall de Crane-Crows come. All on 'em vwas singin'. Crane-Crows vwas say'n, "Hunte man dead to-day; Hunte man dead to-day." Dis Queen Crane-Crow say, "Save 'is eye-ball fur me!" Dey didn' vwan' believe 'e vwas dead. 'E sen' one o' de hudder Crane-Crows to pick 'im.

Dis little Crane-Crow *gone*, 'e pick de man. B' Queen Crane-Crow say, "Pick 'im again!" 'E pick 'im; de man ain' move. Now hall on 'em vwas comin'; begin to pick de man. Vw'en dis Queen Crane-Crow come to pick hout de man heye; de

man hold de Queen Crane-Crow. De Queen Crane-Crow holler, "Tain' me! Tain' me! Tain' me!" De man say, "No good, I got you now, you got to go." 'E put 'im inside 'e bag; 'e carry 'im to de Queen. De Queen give 'im a fortune an' de man vwas rich fur 'is life time. (Dats hall.)

E bo ban, dat story 's en', etc.

De Big Worrum.

Once it vwas a time, etc.

So dis day it vwas a man; he had two sons; dey didn' have no fire. Hall dey had to heat vwas raw potatoes. Now de man sen' dis boy to look for fire. De boy vwalk; he vwalk; he vwalk till vw'en 'e look 'e see one smoke. Vw'en 'e gone 'e git to dat fire. Vw'en 'e get dere, de worrum vwas full o' fire. De boy say, "Dimme some fan!" (Give me some fire). De worrum say, "Tain', tain' none; jes do fur me." De worrum say, "Come in little closer" *Good!* Soon as de boy vwen' a little closer, vw'en 'e vwent to reach de fire de worrum swallow 'im down. Den de boy vwen' down, right down, down inside de worrum till 'e stop. De boy met whole lot o' people vwat de worrum did swallow.

So now the man tell de hudder son, "I wonder whey my son gone!" De hudder son say, "Pa, I goin' look fur him." 'E vwalk, 'e vwalk, 'e vwalk till 'e come to this big worrum vw'at had de fire in his mouth. So now the boy vwent to de worrum. De boy say, "Dimme some fan!" De worrum say, "Keelie o' fire" (Come and get fire). De boy say, "*Do i en e*,[1] dimme some fan?" De worrum say, "Come a little closer." De worrum say, "Time for

[1] A phrase, the original meaning of which, had it any, is lost.

Joe come" (Time to go home). De worrum say, "Keelie o' fire." Vw'en de boy vwen' to get the fire *so*, de worrum swallow him down. De boy vwen'; 'e vwen' down; 'e vwen' down, till 'e met 'e brudder.

Now de boy fadder say, "My two sons gone an' I might as vwell gone too." De man take 'e lan' (lance); it fairly glisten, it so sharp. Vw'en 'e git dere whey de worrum vwas wid de fire in he mouth, de man say, "Dimme some fan?" De worrum say, "You too do fur me!" (You're too much for me). De worrum say, "Keelie o' fire." Vwen de man vwen' to get de fire, *so*, de worrum vwen' to swallow 'im. De man take he' lan'; as 'e vwas goin' down 'e cut de worrum; 'e cut de worrum till 'e cut de worrum right open an' all de people come, an' dat vwas a big city right dere.

E bo ban, dis story 's en', etc.

ON SOME CHARACTERISTICS OF SYMBOLIC LOGIC.

CHRISTINE LADD FRANKLIN.

It is now thirty-five years since the publication of Boole's *Laws of Thought*. It has frequently happened, of course, that a great advance in the work of extending scientific ways of thinking over regions which are still in the hands of the natural man has failed to meet with the recognition that it deserved, but, none the less, every fresh instance of a misfortune of this kind is a fresh subject for regret.

The task which Boole accomplished was the complete solution of the problem:—given any number of statements, involving any number of terms, mixed up indiscriminately in the subjects and the predicates, to eliminate certain of those terms, that is, to see exactly what the statements amount to irrespective of them, and then to manipulate the remaining statements so that they shall read as a description of a certain other chosen term (or terms) standing by itself in a subject or a predicate. Ordinary syllogism consists of elimination in the simplest possible case; when we infer from "all kings are tyrants," and "all tyrants are assassinated," that "all kings are assassinated," what we do is to gather up all the information which is conveyed by both premises together, exclusive of that which concerns tyrants. It is plain that ordinary syllogism is of very little avail when there are several premises of such complexity as this, for instance:—"Every a is one only of the two, x and y, unless it is z or w; in the former of which cases it is both x and y, and in the latter case neither of them.

However great a distaste one may have for Symbolic Logic, and however certain one may be that such complicated states of things as the above sentence describes never arise in nature, it is impossible not to admit that it is a great gift to the powers of the common mind to have devised a method by which one can sit down before half a dozen premises of this kind and know just how to go to work to pick out with absolute certainty everything that is said about anything, with or without allusion to anything else. The unaided mind, with a good deal of struggling and floundering, can do pretty hard work of this kind, if it has unusual powers of concentration, but it cannot do it without extreme effort. An organized method for doing thinking like this (which is simply what Symbolic Logic is) amounts, to say the least, to an immense economy of intellectual work. This problem of Logic was completely solved by Boole.

The underlying process of thought involved in his rule for elimination consists, as might have been predicted, in nothing more nor less than the syllogism; the mental act by which, when a term is so given as to form a safe connecting link between other two terms, that term is dropped and the implied connection between the other two terms is set forth in a single statement, is syllogism and nothing else. The only difficulty which the natural man need feel in cases of any complexity whatever, is to extricate the term to be eliminated from the mass of terms attached to it either conjunctively or disjunctively, and to set it forth by itself in the subject or in the predicate; if he could accomplish that, an ordinary syllogism would suffice to put it to flight. It is this extricating which ordinary logic refuses to concern itself with, and it is this limitation of its scope which is largely the cause of the unreality

and of the remoteness from actual thinking that ordinary logic has in the minds of those who are forced to study it. In everyday life, we have constant occasion for seeing (and we have no difficulty in seeing) the exact equivalence of two such statements as these:

"All students of chemistry are also students of either biology or physics."

"Students of chemistry who do not study physics all study biology."

And yet this is an inference which ordinary Logic takes absolutely no account of. What it amounts to is the changing of a positive disjunctive term in the predicate into a negative conjunctive term in the subject. It is a very simple step for the human mind to take, and yet it is the starting point of the immense command over intricate reasoning which is furnished by the modern developments of Deductive Logic. Common logic itself would gain immensely in scope and freedom, and hence in apparent naturalness, if this process were added to the subjects which it now discusses.

The secret of the great command which Symbolic Logic has over complicated trains of reasoning is wholly contained in the fruitful idea that subject and predicate are not necessarily indivisible wholes, but that they can be broken up and their separate elements shifted at pleasure from one side of the copula to the other. If it were doubted before, by the rigid adherents of Boole, that this is the real essence of the matter, and that the symbolism is merely a convenient, not an essential, tool, it can no longer be doubted since Mr. Keynes (*Studies and Exercises in Formal Logic*) has taken the trouble to write out the whole subject with almost no symbolism at all,— with nothing, in fact, except the absence of a symbol to denote *and* (that is, he writes

a b for *a and b*), and the use of large and small letters to denote positive and negative terms respectively. Any considerable amount of symbolism is therefore not a necessity to the new logic, but it is, all the same, of an immense advantage to it.

But with the introduction of symbols, there suddenly arises a strong feeling of abhorrence in the mind of the regular logician. They change Logic into a branch of mathematics, he says,— and with the suppressed premise that mathematics is something not to be endured. But the charge is a wholly unfounded one. Whether a thing is mathematics or not, depends upon its subject-matter, not upon the accidents of its dress. Every form of deductive reasoning is mathematical in the sense of being highly abstract, and of being subject to formal rules of procedure, which make it possible to carry it on without knowledge of what the things are that one is reasoning about, but it is non-mathematical in the sense that it does not deal with any kind of quantity. It is merely a question of convenience of nomenclature whether one defines mathematics to be that branch of logic which deals with measurable quantities, or logic to be that branch of mathematics which deals only with objects in masses and their qualities, without regard to their number or their size, or whether (what is doubtless preferable) one considers them to be parallel branches of science in general. In any case, the greater or less extent to which the invariable rules by which one *must* reason are shadowed forth by formal rules for the combination of counters which stand for the products of thought, has nothing to do with the question. It happens that mathematics is a subject of such tremendous complexity that it is absolutely necessary for it to have recourse to all sorts of signs and symbols and abbreviations, in order to make it at all

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possible for the mind to grapple with it. It does not follow that every other science which finds its advantage in similar means is by that reason turned into mathematics. Modern chemistry makes use of a far more highly developed symbolism than any logician has ever thought of proposing. If the symbolical representation of chemical compounds had happened to receive a distinct name borrowed from mathematics, if it had been called chemical quaternions, for instance, there is no doubt that it would have awakened much repugnance in the minds of conservative chemists, and that it would have had a long struggle to get itself accepted.

What is a symbolic treatment of a subject? It is nothing but a system of abbreviations, having more or less of a pictorial quality, having more or less the character of *icons*, as Mr. Pierce would say, according as that character may be desirable or attainable. Even language is constantly simplifying itself by making use of abbreviated marks for complicated concepts; we said at one time *a moving mass of men;* then we took a single Latin word, *mobile*, for that complex idea; then we abbreviated that word into *mob*, and at present the word mob is, as far as our ordinary consciousness is concerned, nothing but an arbitrary mark for the longer phrase. The great aim which language and science both have always before them, is to enable us to do the greatest possible amount of thinking with the least amount of trouble consistent with perfect clearness. The first aid which the overburdened mind seized hold upon, when the premises thrust upon it were too involved for its easy grasp, was paper and pencil; and it is quite conceivable that some early purists objected to this substitution of a mechanical device for the noble work of reason. It is certain that the early introduction of the signs and symbols of alge-

bra met with very great opposition. It was long before mathematicians were able to reconcile themselves to writing y^5 for $y\ y\ y\ y\ y$; and it was still longer before they ceased writing $y\ y$ instead of y^2; they said, doubtless, that in the case of the second power, y^2 was just as long in writing as $y\ y$, and that the latter was easier of comprehension. But this was at a period when the human mind was not accustomed to bold devices for facilitating its hold over nature. At present, no true lover of science hesitates to avail himself of any well devised scheme for enabling him (with paper and pencil before him) to condense into a single field of vision as much information as possible. And it is very little creditable to the logicians that they have shown an unreasoning horror of a given macroscopic arrangement, merely because it has shown itself to be serviceable in the hands of the mathematicians.

The logicians, it is true, have had some excuse in the fact that the scheme of symbolical reasoning proposed by Boole was immensely more complicated and involved than there was any occasion for. Great as his contribution to science was, Boole had an exaggerated idea of its mysterious inward significance. If he had made less of it, others would without doubt have made more of it. The complete solution of the problem is in reality extremely simple, both in theory and in mechanical execution. The introduction of "functions" and "developments" and the whole idea of inverse processes, while it formed a natural way of looking at the question for the trained mathematician, was wholly unnecessary, and was well calculated to frighten away the mere logician. The whole difficult procedure of Boole has been rendered superfluous by the simpler systems of later writers.

This is not the opinion of Mr. Venn,— the most

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voluminous of recent writers on the subject. He says (*Symbolic Logic*, p. xxviii): "Other writers have spoken of their 'systems' and contrasted them with that of Boole; but at present there is, I think, only one thing before the world which can without abuse of language be called a system (unless the single methodical alteration of marking alternatives on the non-exclusive plan be allowed to rank as such), and this is exclusively due to Boole."

In order to make it plain that this is a mistaken view, it is necessary to inquire a little more minutely into what it is that constitutes a system of logic. The inventor of a system of Logic has three distinct tasks to perform:

1. He must take the multitudinous propositions which are handed in to him by common language and reduce them all to a limited number of forms of expression.

2. He must lay down the rules in accordance with which several of these statements are to be united into one, or one is to be broken up into several.

3. He must lay down rules in accordance with which information in regard to some of the terms is dropped while absolutely all the information which does not regard those terms is retained.

These three processes may be called, respectively, expression, combination and elimination. (If any peculiar and unnatural form of expression is to be given to the conclusion — as is the case in Boole's system — that would constitute a separate process.) Expression will consist of two parts: a form must be chosen for putting together the separate terms which go to make up the subject or the predicate, and a form must be chosen for attaching the subject and predicate to each other. Terms are put together in actual thinking in two totally different

ways. I mean two different things according as I say:

"All of my friends are *either* learned *or* virtuous." (1)

Or, "All of my friends are *both* learned *and* virtuous." (2)

It is therefore absolutely necessary to have two different signs for connecting the letters, which are to serve us as symbols for terms. These two signs might be anything whatever; the two that are usually employed are, respectively, *the sign of* $+$, and *the absence of any sign*. These are the signs which the mathematician has appropriated to addition and multiplication of quantities, but their use in logic has only a fictitious connection with their use in mathematics. A very large amount of very useless discussion might have been saved if non-mathematical signs had been employed for logic from the start. It is true that in logic a factor is distributed over a sum, as it is in mathematics,

$$a\,(b + c) = a\,b + a\,c,$$

(it is natural to borrow the names *sum*, *factor*, *product*, having borrowed the signs); but so also is a summand distributive over a product,

$$a + b\,c = (a + b)\,(a + c),$$

which is not the case in mathematics. The suggestiveness of the use of the mathematical signs is fully equalled by its deceptiveness.

Nor would the slightest difficulty in the working of the logical algebra be occasioned if the meanings of the signs were interchanged, and if we wrote $a\,b$ for what is either a or b, and $a + b$ for what is both. But the fact remains that there is hardly anything which is easier to write than $+$, and nothing which is easier to write than nothing; and also that to write $a\,b$ for what is both a and b, while it has absolutely no connection with multiplication, does closely

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simulate the device of grammar by which we say *green apples* to indicate those things which have both the qualities of apples and the quality of being green. But whatever two pictures might be chosen to stand for these two kinds of term-attachments, *the rules for their manipulation must always be exactly the same.* Two statements which are identical in meaning, respectively, with the two statements given above are:

"Whoever is *both* unlearned *and* non-virtuous is not my friend." (1)

"Whoever is *either* unlearned *or* non-virtuous is not my friend." (2)

Thus, a conjunctive combination in the predicate of the affirmative proposition becomes disjunctive (and of opposite quality) in the subject, and conversely. In other words, the negative of a sum is a product of negatives, and the negative of a product is a sum of negatives; or, expressed symbolically, $\overline{a + b} = \bar{a}\,\bar{b}$, and $\overline{a\,b} = \bar{a} + \bar{b}$; that is, what is not either a or b is both non-a and non-b, and what is not both a and b is either non-a or non-b. This rule is an immediate consequence of the two properties of the negative (namely, that x and $\bar{x}$ are mutually exclusive and that they together exhaust the universe); and it must appear in exactly this form in every system of logic (regard not being had to the unimportant modifications produced by marking alternatives on the exclusive plan).

Now if a system of logic consisted in the treatment of the aggregation and determination of terms alone, it would be correct to say that Boole's system is the only one that exists; but in that case Boole's system could not be credited to Boole. Mr. Venn has himself pointed out that Lambert (*Logische Abhandlungen*, 1781) described these logical relations by these words and represented them by the signs $+$ and $\times$.

As matter of fact, however, this is only the beginning of a system of logic. A proposition has not been stated until the connection between its subject and predicate has been laid down, and it happens that this connection differs from that of aggregation or determination in the fact that it is *not* uniform. Ordinary language supplies us with a countless number of forms of propositions, and ordinary reason picks its way from one to another of them by instinct. Upon examination, it is found that these multiform propositions consist of eight essentially distinct types. Common logic has long since forced all propositions into the fixed mould,— subject, copula, predicate; and of the eight propositions which De Morgan has shown to be necessary to complete expression, it has admitted four. The four which it has not discussed may be got from those which it does recognize by the admission of negative subjects; and since De Morgan's great contribution to logic of the idea of a limited universe, negative subjects ought not to seem to the logicians to be too difficult for discussion. In real life, of course, they are as natural as possible,—"Who will not work, must starve," "What is not cheap is not always good,"— and we have no more difficulty in making real syllogisms out of them than out of the types that are recognized.[1]

Another way of seeing the necessity for their admission is this: every proposition (whatever other import it may have) asserts the existence or the non-existence of a certain combination of qualities;

[1] One of the two propositions with negative subjects can be discussed by the unreformed logician in its contraposed form,— "Those who could not swim were not saved" is the same thing as, "All who were saved could swim,"— but the other, "Whoever is not virtuous is miserable," has for its contraposed form "Whoever is not miserable is virtuous," and the subject is as badly negative as before. The ordinary logic has absolutely incapacitated itself from admitting either of these last two sentences into its scheme.

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"all a is b" says that there is no such thing as the combination $a\,\bar{b}$, and "some a is not b" asserts that objects do exist which have the qualities a and $\bar{b}$. But all the combinations of a and b (with their negatives) are four in number,—$a\,b$, $\bar{a}\,\bar{b}$, $\bar{a}\,b$, and $a\,\bar{b}$,—and when the signification of the letters is not known, there is no reason for supposing that a certain two of them have any sort of superiority over the other two. It will therefore take eight propositions to affirm and to deny their existence. But by choosing a proper form of words (that is, by throwing the expression of the relation between the terms virtually into the copula), all these eight different things can be said in terms of a and b only,—that is, without the admission of negative terms at all. The following table gives the four different universal statements that can be made in terms of a and b, together with the four denials of them, which are particular propositions.

FOUR DIFFERENT STATEMENTS OF FACT.

UNIVERSAL.

NON-SYMMETRICAL.	(A) All of a is b. All of $\bar{b}$ is $\bar{a}$.	(V) None but a is b. None but $\bar{b}$ is $\bar{a}$.	(E) None of a is b. $(ab)_0$	(Ɜ) All but a is b. $^\infty(a+b)$	SYMMETRICAL.
	(a) Not all of a is b. Not all of $\bar{b}$ is $\bar{a}$.	(v) Some besides a is b. Some besides $\bar{b}$ is $\bar{a}$.	(e) Some of a is b.	(ə) Not all but a is b.	

PARTICULAR.

I have elsewhere suggested copula signs for these eight propositions, which indicate by their several characters whether the proposition which they symbolize is universal or particular, positive or negative, symmetrical or non-symmetrical; and which are such that a simple rule shows how to

transform a given proposition from one form into any other.[1] It will be noticed that the four propositions of the right-hand half of the table can be read backwards as well as forwards,—"all but a is b" is the same thing as "all but b is a;" but that is not the case in the left-hand half of the table,—"none but a is b" is not the same thing as "none but b is a," but it is the same thing as "none but $\bar{b}$ is $\bar{a}$," or, on changing the places of the terms their quality must be changed also, in order to get an equivalent statement. We shall express this by saying that the copulas of the left-hand half are non-symmetrical and those of the right-hand half are symmetrical. Of the symmetrical copulas, one can be inserted anywhere in a product, and the other can be inserted anywhere in a sum; "none of $a\ b$ is $c\,(b + d)\ e$" is identical with "none of $a\ b\ c\ (b + d)$ is e," and "all but a is $b\ c + d\ e\ f$" is identical with "all but $a + b\ c$ is $d\ e\ f$," with the same thing for the corresponding particular propositions. Moreover, with the symmetrical forms of expression, nothing is changed by putting all the terms into the subject or the predicate as the case may be, thus: "no a is b" is the same thing as "a which is b is nothing," and may be written, upon occasion, $(a\ b)_0$; and "all but a is b" is the same thing as "everything is either a or b," and may be written, upon occasion, $x(a + b)$.

The table already given exhibits the four different forms of expression for four different statements of fact. The four universal (or the four particular) copulas may be made to give expression to one and the same fact by attaching the proper quality to the terms which enter them. Thus, in the following table, all of the universal propositions are equiva-

[1] Some Proposed Reforms in Common Logic. To appear in *Mind*, January, 1890.

lent in meaning with "no a is b," and all of the particular propositions are equivalent in meaning with "some a is b."

FOUR DIFFERENT FORMS OF EXPRESSION FOR ONE AND THE SAME STATEMENT OF FACT.

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NON-SYMMETRICAL.	(A) All of a is $\bar{b}$. All of b is $\bar{a}$.	(V) None but $\bar{a}$ is b. None but $\bar{b}$ is a.	(E) None of a is b. $(a\ b)_0$	(Ǝ) All but $\bar{a}$ is $\bar{b}$. $\infty(\bar{a}+\bar{b})$	SYMMETRICAL.
	(a) Not all of a is $\bar{b}$. Not all of b is $\bar{a}$.	(ɐ) Some besides $\bar{a}$ is b. Some besides $\bar{b}$ is a.	(e) Some of a is b.	(ə) Not all but $\bar{a}$ is b.	

PARTICULAR.

In other words, the fact, "no a is b," can be expressed, at pleasure, in terms of a and b, $\bar{a}$ and $\bar{b}$, a and $\bar{b}$, or $\bar{a}$ and b; and the same is true of the fact, "some a is b."

But the first requirement of a good working scheme is that it should admit no more variety of expression than is absolutely necessary. A single fact, instead of being expressed, at pleasure, in four different ways, as is done in real life, must be expressed in one way only. *Now the whole subsequent working of a scheme of logic will be totally different according as one or the other of these four different modes of expression is chosen as the normal form.*

For one thing, if all propositions are expressed in either of the symmetrical forms, there will be no distinction between subjects and predicates, and both will be treated in accordance with exactly the same rules; but if either of the non-symmetrical forms is chosen, the rules for subjects and predicates will be, throughout, the reverse of each other. Take, for

instance, the rules for under-statement. From "all of $a + b$ is $c\,d$" it can be inferred that "all of $b\,x$ is $c + y$" (where x and y are anything whatever), but not that "all of $a + b + y$ is $c\ d\ x$;" and the same thing holds for the corresponding particular proposition, "not all of a is b," or "some of a is not b." The rule is, that, with this copula (and its negative) an addend can be dropped or a factor can be inserted in the subject, and a factor can be dropped or an addend inserted in the predicate,—a difficult rule to remember. With the other non-symmetrical copula, the rule is exactly the opposite of this. With either of the symmetrical copulas, only one thing has to be borne in mind for the whole complexus of terms. The combination of two universal propositions is got by adding their terms if the 0-copula is used, and by multiplying them together if the ∞-copula is used; to say that there is no $a\,b$ and at the same time there is no $c\,d$ is the same thing as to say that $(a\ b + c\ d)_0$ but to say that everything is either a or b and at the same time everything is either c or d is to say that ${}_\infty(a + b)$ $(c + d)$. With the non-symmetrical copulas, statements cannot be combined at all (without loss of content) except by virtual transformation into one of the symmetrical copulas, unless they happen to have like predicates or like subjects. The process of elimination presents corresponding differences of treatment with different copulas.

It is plain that the several copulas are as unlike as light and darkness. The symmetrical copulas have the immense advantage, in point of simplicity, that all the terms which they connect play the same role. The second non-symmetrical copula is merely the obverse of the first and does not invite separate discussion. The first, "all a is b," has the important consideration in its favor that the propositions pre-

sented by nature to the logician are most frequently already in that form. This fact lends an apparent naturalness to this form of expression, and if only questions of moderate difficulty are to be attacked, it might be regarded as a determining consideration. The rules for working with this copula ought to be laid down in any treatise on the subject, because they are the generalization of the affirmative syllogism in the first figure, and the first figure has always been the peculiar favorite of the logician. But when the premises are so complicated that a great deal of transposing between subject and predicate has to be done anyway, it is unwise to strain at the slight additional unnaturalness involved in putting all the terms into the subject or into the predicate, in view of the large amount of mental energy that is set free by the fact that one no longer has to bear in mind a different mode of treatment for terms, according to the place in which they occur.

When Mr. Venn said that there is only one system of Logic, he seems, from a foot-note, to have had in mind only Jevons as a pretended maker of another system; and Jevons's work in Symbolic Logic does certainly not amount to a system, but merely to the absence of a system. Since then Mr. Keynes has published his treatment of the non-symmetrical copula, and he states in his preface that he is peculiarly indebted to Mr. Venn—"not merely by reason of his published writings, but also for most valuable suggestions and criticisms given to me while this book was in progress." From this it might, perhaps, have been inferred that Mr. Venn is now aware that the most difficult problems of Symbolic Logic can be solved with other copulas than the negative symmetrical one, were it not that his latest utterance on the subject is this (*Empirical*

Logic, 1889, p. 230): "In fact, groups of really complicated propositions cannot easily be combined, and their net result completely determined, on any other scheme yet worked out." This, it is true, is a much more guarded statement than his former one, and it is also true that complete combination is less easily effected with either of the non-symmetrical copulas than with the others; but the two symmetrical copulas are exactly on an equality as respects facility of complete combination, and the Logic of the affirmative one was completely worked out, six years ago, by Mr. Mitchell. Mr. Keynes also gives (without noticing its importance) all that is really necessary to the Logic of (universal) propositions beginning in "everything is."

Since the character of a system of Logic is absolutely determined by the character of the copula which is chosen to represent its propositions, it follows that there are four essentially different systems of Logic possible,—provided the form chosen for the expression of the particular proposition is the simple denial of the universal, that is, provided one were always to combine two propositions taken from the same column of the Table. But this is not necessary — it may happen that some advantage is gained by putting together a particular of one couplet and a universal chosen from another, and, as matter of fact, it has happened that a very great advantage has been gained by that means. With this degree of freedom, there are sixteen possible systems of Logic, namely, any one of the four universal propositions, in combination with any one of the four particular propositions, may be taken as the standard form of expression. Have all of these sixteen forms of Symbolic Logic been worked out? Virtually, they have,— or, at least, all which could be of any interest,— and it is therefore no longer a possibility

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for any one to invent a new system of Logic.

1. The copula chosen by Boole was the negative symmetrical copula, "no a is b," or "there is no a b," which he wrote $a\,b = 0$. This statement is really of the nature of a proposition, "a which is b *is* non-existent," not of an equation, and it happens that the equational form of expression, while not close to the real nature of the thing to be expressed, had the unfortunate effect of making people think the subject more mathematical than it was, and hence more unpleasant. For a particular proposition, Boole took the immediate denial of the universal,—"some a is b;" but the Logic of the complex particular proposition, and of its combinations (conjunctive and disjunctive) with the universal proposition, he did not work out at all. Boole's further method of procedure is deserving of great credit as a first attempt, but it is cumbrous and (to the non-mathematician) mysterious to the last degree. The final form has been given to the Logic of this copula by Schröder,[1] and his treatment of the subject ought to have completely superseded that of Boole. That it has not done so is doubtless owing to the accident that he has not yet had an English commentator. Mr. Venn makes some allusion to Schröder, but only to his adoption of the non-exclusive plan for logical addition, which is an unessential, though praiseworthy, feature of his method. It is very singular that Mr. Venn does not seem to be aware that the problem of Logic has been completely and admirably worked out by Schröder. His rule for elimination amounts to exactly the same thing as Boole's, from the necessity of the case, and yet Mr. Venn can say, of Boole's formula for elimination, that it "does not seem to be thus capable of introduction [*i. e.* with

[1] Der Operationskreis der Logikkalkuls. Leipzig, 1877.

non-exclusive addition], except under restrictions which almost amount to doing without it altogether." Schröder has accomplished in a few pages, and with admirable simplicity and closeness to real processes of reasoning, what Boole made a very obscure and tortuous journey of. The teacher of Logic who still thinks it necessary to expound the laborious methods of Boole, for any other than historical purposes, is doing his students a serious injury.

I have added the logic of the corresponding particular proposition[1] to Schröder's development of the logic of "no *a* is *b*." The subject is left in a very incomplete state as long as the particular proposition remains undiscussed. Mr. Keynes says (*Formal Logic*, p. 313), "Particular propositions are not in themselves of great value; and they may involve us in troublesome questions of 'existence'." But a universal proposition cannot be simply denied except by means of a particular, and that would be a singular world of argument in which no one was ever permitted to enter a discussion who wished to deny anything which had once been said. Particular propositions do not, in real life, often give us information which is interesting in itself, from a scientific point of view; but they do constantly give us information which is of very great importance in guarding us from a belief in universals which had been supposed to be true. Whoever had been in danger of basing conduct on the premise, "all Catholics are followers of Anti-Christ," would, logically, be very much influenced by the discovery that "some Catholics are saints."

Nor is there anything necessarily troublesome in the question of existence. The import of the particular proposition, "some *a* is *b*," is to affirm the

[1]Studies in Logic, by Members of the Johns Hopkins University. Boston, 1883, pp. 17–71.

existence of the compound ab, and hence of its elements, a and b. Particular propositions, therefore, always affirm the existence of their terms (or of an alternation of their terms) when expressed in this way. The import of the universal proposition is, in every case, to deny the existence of a compound, but not necessarily to affirm the existence or the non-existence of its elements. If, in a particular case, it is known, by extraneous considerations, that the existence of the subject is actually implied, that fact can be stated (and reasoned upon) as a separate proposition,—namely a particular proposition, an affirmation of existence, "there are some a's." All the consequences of these conventions in regard to existence are perfectly easily carried out, and any algebra, therefore, which does not provide for the introduction of particulars,—that is, which does not permit the denial of universals—is thoroughly and unnecessarily incomplete.

The copula "no a is b" might have been combined with any one of the three other particular forms of expression. Its combination with "not everything but a is b," instead of with "some a is b," would give rise to an algebra which would be the exact dualistic of Mr. Mitchell's algebra,—or, it would be exactly like it, if the *meanings* of the symbols for addition and multiplication were interchanged. Its combination with either of the non-symmetrical copulas would lead to much confusion in rules, without any compensating advantage. The syllogisms,

No x is p,
Not only s is x,
Not only p is non-s,

and

No p is non-x,
Not only x is s,
Not only p is s,

will illustrate the rules for elimination with this combination of copulas.

2. The logic of the non-symmetrical affirmative copula, "all a is b," was first worked out by Mr. Maccoll[1]. Nothing is stranger, in the recent history of Logic in England, than the non-recognition which has befallen the writings of this author. The fact that his contributions appeared in a journal which logicians were not in the habit of referring to (his brief article in *Mind* did not do his method justice), the fact that he was not acquainted with the writings of Boole, and the further accident that he considered it a matter of importance to read "all a is b," which he wrote $a : b$, in the words, "the statement that a thing is a implies the statement that it is b,"—all doubtless contributed to making him seem foreign to writers trained in the usual schools of logic. The fact that the nature of the connection between the terms in "all x is y," and between the propositions in "a is b is-always-followed-by c is d," is exactly the same, and must be exhibited in the same formal rules of procedure, has nothing to do with the *words* in which the proposition and the sequence may be expressed. But if it had not been for this accidental misfortune, it seems incredible that English logicians should not have seen that the entire task accomplished by Boole has been accomplished by Maccoll with far greater conciseness, simplicity and elegance; and, what is an interesting point, in terms of that copula which is of by far the most frequent use in daily life.

Mr. Keynes has re-written the entire logic of this copula, (with the unimportant modification that he prefers to use the printed word *or* instead of the sign $+$), *without so much as a single allusion to Mr. Maccoll*. Mr. Venn says (*Symbolic Logic*, page 372, foot-note): "After a careful study [of this

[1]Proc. London Math. Soc., Vols. IX., X., XI., 1877-80.

scheme], aided by a long correspondence with the author, I am unable to find much more in it than the introduction of one more scheme of notation to express certain modifications and simplifications of a part of Boole's system." But Mr. Maccoll has completely solved the problem of logic,—to throw the multiform propositions of real life into a single standard form of expression, to condense the information that interests us by the elimination of certain terms which we do not care for, and to state the information which is left in the form of any terms which we happen to wish to see described. The part of Boole's Logic which Mr. Maccoll does not discuss is the inverse operations; but they are an entirely unessential part of the scheme. If anyone cares to interest himself in them, as a matter of idle curiosity, it should be done in an appendix or a foot-note; even in Boole they work merely to obscure the real process of thought, and the actual reasoning, whether formal or not, goes on its way, entirely oblivious of their existence.

Mr. Maccoll chooses for the expression of his particular propositions the simple denial of his universals, and he writes them, very properly, with the sign of negation attached to the affirmative copula; but he does not discuss their treatment in cases of any complexity. Neither does Mr. Pierce, who has worked out independently the Logic of the same copula—at least, his Fourth Process [1], which is the rule for elimination, concerns only universal propositions, and his Sixth Process is not true of both, unless "taken together" were to mean *taken in combination if they are universal and taken in alternation if they are particular*. The combination of the universal "all non-a is b" with "not only a is

[1] Am. Jour. of Mathematics, Vol III. (1880), p. 39.

non-b" as its denial, would give rise to an algebra in which the rules for elimination between a universal and a particular proposition would be exactly the same as for that between two universals. This universal combined with either of the symmetrical particulars would not be interesting.

3. The negative non-symmetrical copula, "none but a is b," would give rise to an algebra in which everything would be exactly the reverse of the algebra of "all a is b."

4. There remains the algebra of the affirmative symmetrical copula,—"all but a is b," or, "everything is either a or b." This was admirably worked out by Mr. Mitchell, the late Professor of Mathematics in Marietta College, when a fellow in the Johns Hopkins University[1]. His algebra would have been exactly like the algebra of "no a is b," with the exception that everywhere addition and multiplication were interchanged, had he not had, for the first time, the extremely happy idea of combining with his universal proposition a particular taken from a different rubric,—namely, of working out the algebra of the combination,

Everything is a or b,
Something is $\bar{a}$ b.

By this device, the rules for combination and elimination (where they can be effected at all) *are all identically the same, whether the propositions are universal or particular*. This circumstance gives this algebra a very great advantage over any which seeks to combine a universal proposition with its immediate denial as the particular. It is an advantage which is shared by the combination.

No a is b,
Not all but $\bar{a}$ is $\bar{b}$.

[1] Studies in Logic, pp. 72–106.

and, for simple propositions, this would work about as well as that; but for compound propositions, which we now proceed to consider, it happens that this latter couplet of forms of expression is decidedly unnatural.

The four types of universal compound proposition, when completely expressed, are (if P and Q stand for propositions),

A. If P is true, Q is true,
Y. Only when P is true is Q true.
E. Never when P is true is Q true.
I. Unless P is true, Q is true,
or, 'The possible' implies that
P is true or that Q is true.

A and *E* are the commonest forms of speech for simple propositions, but, by a very curious accident of language, it is not *A* and *E* but *A* and *I* that are the chosen forms for compound propositions, or sequences, as they may be called, and so great is their preponderance that we have an elliptical form of speech for them,—"if *P*, *Q*," and "either *P* or *Q*," (that is, "if *a* is *b*, *c* is *d*" and "either *a* is *b*, or *c* is d.") They are, moreover, strange as it may seem, the only forms of the compound proposition which are ever treated by the logician, and so great is the apparent difficulty of these forms even, to one who has not been trained in Symbolic Logic, that a very recent writer of a text-book actually supposes that

Either *A* is *B* or *C* is *D*,

and

Either *A* is *B* or *C* is sometimes not *D*,

are propositions which are denials each of the other.

The actual denials of all four ways of saying one and the same thing are as follows (they are, of course, *particular* sequences):

UNIVERSAL.	PARTICULAR.
I. Either some must dance or all must sing.	o. 'T is not necessary that some should dance or else all sing.
A. If none dance, all must sing.	a. If none dance, not all need sing.
V. Only if some dance may some not sing.	u. Not only when some dance may some not sing.
E. 'T is not permitted that none dance when some do not sing.	e. None may dance *and* some may not sing.

The *words* to be chosen are different according as the sequence is matter of logical or merely material following—in the one case we prefer, for instance, to begin A with *if*, in the other with *when*; but the *connection* between the two propositions is, in both cases, of the same formal kind, and subject to the same symbolical treatment. Of Mr. Mitchell's coupet of sequences,

> *I*. Either no *a* is *b* or some *c* is *d*.
> *e*. Sometimes some *a* is *b* and no *c* is *d*,
> or, It may be that
> some *a* is *b* *and* that no *c* is *d*,

it happens that the universal is one of the two most natural, and the particular is the very most natural of them all. That is not, of course, true of his simple propositions; "everything is either not a king else a tyrant" is the least frequently natural of all the forms of expression. It would be interesting to explain, if it could be done, why the relations of terms and the relations of propositions have fallen, in real life, into such different grooves as regards expression.

What I hope to have shown is that two systems of logic are not made the same system by the fact that both are systematic methods of procedure, nor yet by the fact that both express the common part and the aggregate of two terms in the same way; that two systems which have in every respect different rules of procedure, basedpon the fundamental tal differences of their several copulas, are, in any

natural sense of the phrase, different systems; that there are sixteen different forms of symbolic logic possible, the combinations of any one of the four universal propositions with any one of the four particular; that eight of them (the combinations of a symmetrical with a non-symmetrical form) are wholly uninteresting—that is, they occasion confusion without securing naturalness; that, of the other eight, what suggests itself most naturally is the combination of each universal with the particular which immediately denies it, but that the combination with the *other* particular of the same symmetry has the extreme advantage, as a working method, of offering one single set of rules for dealing with particulars and universals; that no system of logic is complete which does not take account of particular propositions; that Mr. Mitchell's system of logic combines naturalness with facility of manipulation (for compound as well as simple propositions) in a higher degree than is possible to any other system; and, in the first place, that the lack of interest which logicians are content to feel in Symbolic Logic is unreasonable, and based upon a mistaken conception of its nature.

MEMORY, HISTORICALLY AND EXPERIMENTALLY CONSIDERED.

W. H. BURNHAM, PH. D.

IV.

I.—*Recent Theories.*

The tendency of recent studies in physiology and pathology has been to establish the importance of physiological processes in all acts of retention and reproduction. Such studies have shown that, whatever be the ultimate relation of mind to body, memory is dependent upon physical processes. A cerebral process of some kind is the physical concomitant of an idea, and the condition of the reproduction of the idea is the repetition of the original cerebral process. In some way the brain centers are modified by impressions; they retain in growth the form of their modifications; and, on occasion of appropriate stimuli, they tend to repeat processes that have once occurred. The following passage from a recent article by a French psychologist illustrates some of the facts that enforce this view: "It is evident that there is in memory something automatic, capable of functioning alone. Even the diseases and illusions to which it is subject prove that there is something delicate and fragile in this marvel of natural mechanism. If a scholar, after having received a violent blow on the head, forgets all his knowledge of Greek without forgetting anything else, and if afterwards, as the result of a second blow, he suddenly regains his lost Greek, it is difficult to see in memory, with M. Ravaisson, an

act entirely spiritual. The automatic side of memory, especially of 'passive memory,' is illustrated by certain remarkable facts, where things have been preserved and reproduced with a facility that is at once perceived to be mechanical. When an imbecile in the asylum at Earlswood can repeat accurately a page of any book read years before and even without understanding it; when another person can repeat backwards what he reads as if he had under his eyes a 'photographic copy of the impressions received'; when Zukertort plays blindfolded twenty games of chess at once without considering anything but the imaginary chess-boards; when Gustave Doré or Horace Vernet, after attentively regarding their model, paint its portrait from memory; when another painter copies from memory Rubens' Martyrdom of St. Peter with an accuracy to deceive the experts, we conjecture that such accurate preservation and reproduction of the impressions received must have their causes in the organs."[1]

Some, however, among recent writers vigorously oppose any physical theory of memory. For example, Prof. Borden P. Bowne, the eminent American disciple of Lotze, maintains that "a physical explanation of memory cannot be found," and that "a mental explanation is equally impossible." Nevertheless, in some mysterious way the soul in its process of perpetual change "carries its past with it; not, however, in the form of latent modifications, but solely in the power of reproducing that past in consciousness."[2] Others, while emphasizing the

[1] From Fouillée's Le mechanisme de la mémoire, Revue des Deux Mondes, 15 Mai, 1885, pp. 359, 360.

[2] Metaph., p. 426. Ibid, p. 427. For a fuller account of Prof. Bowne's position, and a criticism of cerebral theories of reproduction, see Introduction to Psych. Theory, Ch. III, and Appendix to the same. Cf., also Ferri: La psychologie de l' association depuis Hobbes jusqu'à nos jours; and Huber: Das Gedächtniss.

importance of physical mechanism in retention and reproduction, maintain that the higher acts of conscious memory cannot be explained by physical processes. Thus Prof. Ladd, who holds that "the physical basis of memory as retentive is laid in the habit, or acquired tendency, of the elements of the nervous system,"[1] maintains that no physical conditions immediately concern the "mental activity which constitutes the essence of *conscious* memory."

"What is explained, if anything, is simply why I remember one thing rather than another — *granted the mind's power to remember at all.* This power is a spiritual activity wholly *sui generis*, and incapable of being conceived of as flowing out of any physical condition or mode of energy whatever."[2]

Fouillée also, after showing the dependence of memory upon physical processes, emphasizes the part that consciousness plays in a complete act of memory. He writes: "Not only consciousness, in its reflective form, has thus the power of reacting on the conservation and mechanical association of ideas, but also it is absolutely necessary to that third function which is the true characteristic of mental memory, the recognition of what is recalled. The automatism which we have described explains merely the revival of similar ideas and not their recognition as similar.... Memory is indivisibly physical and mental, physical for the spectator from without, mental for the spectator from within."[3]

Few if any will now be found to deny the dependence of memory in some degree upon cerebral processes. Even Prof. Bowne, with all his hostility to physiological theories of memory, admits that

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[1] See Elements of Phys. Psych., p. 552.
[2] Ibid, p. 556.
[3] Loc. cit., pp. 388, 389.

"the brain conditions the mental activities of thought and recollection."[1] The ultimate nature of memory, especially in its function of recognition, being an inscrutable mystery, its physical side then appears the promising field for study. At all events most of the valuable contributions to the psychology of memory in recent years have been made by students in this field. Some of the theories that have resulted from their studies may be briefly mentioned here.

Accepting then the theory that memory — at least as retentive and reproductive — has a physiological basis, what is the physical mechanism that makes the persistence of an impression possible? This is a question that in recent years has divided physiologists. Three theories have been advocated. It is supposed (1) that memory depends upon a *movement* persisting in the brain; (2) that it depends upon a persisting *trace* or *residuum* in the brain; (3) that it depends upon a *disposition* persisting in the brain.

The theory that memory depends upon persisting movement has recently been advocated by Mr. Luys.[2] He calls that property of the nervous elements by virtue of which vibrations in them persist *phosphorescence*, since it seems analogous to that property of phosphorescent substances which causes them to remain luminous after the source of their light has disappeared. Luminous vibrations, as shown by the investigations of Niepce de Saint Victor, may be garnered up in a sheet of paper and by the use of proper re-agents, made to appear months afterward. The same persistence of vibrations is illustrated in the common forms of photography.

[1] Introduction to Psychological Theory, p. 113.
[2] The Brain and its Functions, Book II.

A plate of dry collodion retains for weeks the changes produced in it by exposure for a few seconds to the sun's rays. Similarly in memory, vibrations of an impression persist in the nervous elements, and to revive the impression it is only necessary that a suitable awakening agency renew the vibrations.[1]

The second theory, that memory depends upon a persisting trace, has long been a favorite one. Plato taught it in his figurative way. And among the moderns it has not lacked adherents since the days of the physiologist Haller. Among recent writers Oliver Wendell Holmes has eloquently illustrated it.[2] He deems the hypothesis probable, "that memory is a material record; that the brain is scarred and seamed with infinitesimal hieroglyphics, as the features are engraved with the traces of thought and passion."[3] A favorite illustration of this view has been the comparison of the brain in memory to a phonograph.[4] Like the tinfoil of the latter, the brain preserves a trace of impressions made upon it, and may be called a "conscious phonograph."[5]

Richet has advocated a form of the same theory,[6] and maintained that there is a great difference between the muscles and the nerves, since the effect

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[1] This theory merges into the second; and some writers do not clearly state which is their own view. Draper's illustrations may be taken in connection with either theory. See Conflict between Science and Religion, p. 132, seq.; also, Human Physiology, ch. xxi. The examples from photography also are often used to illustrate the second theory.

[2] Cf. Mechanism in Thought and Morals, p. 69 seq.

[3] Ibid., p. 77.

[4] See The Secret of a Good Memory by Dr. Mortimer-Granville; also an article by M. Guyau, La Mémoire et le Phonographe, Revue Philosophique, March, 1880.

[5] M. Guyau, loc. cit., p. 322.

[6] Les origines et les modalités de la mémoire, Revue Philosophique, June, 1886.

of stimulation upon the former is transient, while upon the latter it is permanent. "In a word," he says, "while the muscles and the organic nerve-cell return completely to their original condition after an excitation, the psychic nerve-cell does not. It has been modified in a permanent manner by the act of stimulation; and this modification can be effaced only by the death of the cell. Each excitation has, so to speak, created a new cell different from the first." [1]

The third view — that the essence of memory is a functional disposition persisting in the brain — is, perhaps, the one most widely held by contemporary psychologists. Among recent writers Wundt[2] and Ribot[3] have been the most prominent exponents of it. But it is not new. Descartes taught it in a dim way.[4] Malebranche[5] and Bonnet[6] held substantially the same view, except that they thought of the disposition as persisting in the nerve fibres, while modern physiologists attribute the storing up of impressions especially to the nerve cells. Hartley's theory of vibratiuncules reminds one of Aristotle and seems to belong in our first class; but he also speaks of the persisting dispositions to these diminutive vibrations;[7] and some of his disciples appear to have held the same view. The theory has already been mentioned, but it may be summed up as follows: When a nerve cell, or a group of cells, receives an impression and reacts, it is modified in some way. There is, perhaps, a change in the molecular structure of

[1] Ibid., p. 565.
[2] See his Grundzüge d. phys. Psych., Bd. II, ch. 17, p. 306 seq. 2te Aufl.; also my 2d article, p. 261.
[3] See Diseases of Memory, Ch. 1.
[4] Cf. my 1st article, p. 66.
[5] Cf. Recherche de la Vérité, II, v, 3; also my 1st article, p. 67.
[6] Cf. Analyse abrégée, ¶ X; also my 1st article, p. 79.
[7] See my 1st article, p. 82.

the cell—of what sort we do not know. It involves, however, an aptitude or disposition for reacting again in the same way. Just as the eye that is constantly practiced in comparing and measuring acquires an aptitude or skill that makes it far superior to the untrained eye. This is a functional, not an organic, change in the eye. In like manner every state of consciousness affects the nervous elements of the brain, and the nerve center is functionally modified so that the recurrence of the same mental state is rendered easier.

Ribot lays stress also upon the establishment of dynamic associations between the nerve elements.[1] The basis of memory, in his opinion, consists not only in the modifications effected in the individual elements, but also in the way various elements are grouped together to form a complex. In so simple a matter as the recollection of an apple, for example, these associations are necessary; for the recollection of an apple is the repetition in weaker form of the perception of the apple. This perception involves not only the retina and transmission along an extended route to the cortex, but association with the motor centers that control the movements of the eye; for it is owing to the latter that we perceive the apple as a solid body. The process of education renders stable these dynamic associations; and "a rich and well-stored memory is not a collection of impressions, but an assemblage of dynamic associations, very stable and very readily called forth."

These three theories, as held by recent writers, are closely related and merge in one another. The

[1] Loc. cit. Cp. also Wundt, loc. cit. These associations depend upon the general law, that when a nervous discharge has occurred along a certain path, discharge along the same path is rendered easier.

movement and the trace may cause the functional disposition. The functional disposition itself may, in the last resort, be due to persisting movement. At present it is impossible to say just what does occur. But the third theory, allying as it does the mechanism of memory to well-known physiological facts, and providing for the retention in growth and transmission to new cells, by a kind of heredity, of these acquired tendencies, presents fewest difficulties, and explains the facts perhaps as well as any.

In the second place, as has been often shown by recent writers, studies in physiology and pathology indicate that we have not memory, but memories. Each sense and organ of the body may be said to have its memory; and these memories are in a measure independent of each other. The visual memory may be lost without affecting the auditory memory. The memory of speech may be impaired and the general motor memories remain intact; and so on. Loss of any of these memories is due to disease in the appropriate center in the brain. Thus it appears that instead of one seat of memory as the older psychologists supposed, the different areas of the brain not only have their special office to perform in governing the organs of the body, but they are also the seat of the respective memories of those organs.[1]

[1] While many of the doctrines of localization are still disputed, few physiologists will deny that our various memories are scattered over the brain, the percipient organ, whatever it is, being also the remembering organ. Apart from physiology and the consideration just mentioned, the view has also been held quite frequently that we have memories rather than a faculty of universal memory. Cardinal Newman writes: "In real fact memory, as a talent, is not one indivisible faculty, but a power of retaining and recalling the past in this or that department of our experience, not in any whatever. Two memories, which are both specially retentive, may also be incommensurate. . . . . There are a hundred memories as there are a hundred virtues." Grammar of Assent, Am. ed., New York, 1870, pp. 327, 328.

The cases where men and animals have lost certain memories and then recovered them may sometimes be explained according to the hypotheses of Wundt and others, as follows: Around that area of the brain which functions any sense or movement is an area that might assume the same office. It is a *potential* area for the memories of that center. For when the actual center of any sense is destroyed a part of this potential area may be educated, so to speak, to perform the necessary functions. For example, if the actual sight area in the posterior part of a dog's brain be cut out, and the potential area remains, the dog becomes blind, but may recover. If, however, the potential area also be destroyed, the dog remains permanently blind.[1]

From what has been said the contrast will be clear between the old view, which made memory a distinct faculty and located it in a definite part of the brain and the modern doctrine of physiology which tends to consider memory as a function of all the cerebral centers.

The next problem that physiology has attempted to solve is in regard to the physical mechanism of the association of ideas. Psychologists from Aristotle down have generally been content with stating the phenomena of association. Modern physiological psychology asks: "Why do ideas recall each other according to the laws of contiguity, similarity, and contrast?"

Fouillée, following Herbert Spencer, has attempted to answer this question. In his opinion the two laws of similarity and contiguity[2] are, as far at least as the physical side is concerned, two expressions of the

[1] Cf. an excellent article by Dr. M. Allen Starr, "Where and How We Remember," Pop. Sci. Monthly, Sept., 1884. Some, however, maintain that the sight-center is not located entirely in the cortex.

[2] Contrast may be omitted as it is a subordinate principle.

same law. When two ideas are associated by contiguity, the physical mechanism is this: Two nerve-currents are aroused in neighboring parts of the brain. They are diffused according to the law of the persistence of movement until they meet and commingle. This union is the germ of habit or unconscious memory. It can, of course, take place only in case of nerve-currents in contiguous parts of the brain. The mechanism for association of similar ideas is the same; for adjacent parts of the brain are similar. Thus Fouillée agrees with Spencer that the prime law of association is this: "*Every presentation tends to associate with similar presentations on account of the identity of their seat in the brain.*"[1] All other laws are subordinate.

"Contiguity in time then," he says, "links things only by means of a contiguity in the extension of the brain. Thus are established in the nerve paths, as on the railroads, junctions, analogous to those where the switchman determines the course of the trains. The succession of ideas, even of those which we recognize afterwards as similar, is caused by the meeting, at the point of bifurcation, of two trains of images in contiguous parts of the brain. The words *entremets*, *entrecote*, *entrepont*, will mutually arouse each other by their point of bifurcation *entre*, and in some diseases the patient repeats these words mechanically one after the other." The cementing power is the persistence of force and the continuance of movement which communicates itself always to adjacent parts. All movement expends itself in one way or another. It cannot stop then, in a group of brain cells, but necessarily passes to neighboring groups, and persists even to the most distant groups through the connection of the association fibres.

[1] Loc. cit., p. 381.

Fouillée criticises Spencer, however, for confusing the mechanical process of joining similar images with the consciousness of their similarity. The latter is a mental synthesis. It occurs only between impressions presented automatically, but should be distinguished from the mechanical process of presenting such impressions. The reacting and synthesizing work of consciousness should be noted, especially in the complicated operation of recognizing the similarity between the past and the present. His position is summed up in the following passage:

"There is no contiguity, in our opinion, without similarity. Objectively, contiguity itself is a kind of similarity, under the relation of space and time; for it is a meeting in the same space and the same time which always tends to a certain fusion of movements the most opposite into a common form of movement. Subjectively, contiguity always becomes a certain similarity in consciousness. The mere fact of perceiving that disparate things *coincide*, as a bright light, a sound, a pain, is already a consciousness of similarity in the very bosom of difference. This judgment involves a reaction of consciousness in respect to sensations coming to it; and it is this reaction which constitutes the mental synthesis. This synthesis, without doubt, can occur only between terms already given by pure automatism, but consciousness perfects and renders permanent the cementing begun by simple mechanical coincidence. . . . . The brain takes note only of contiguity, of which similarity is a consequence; intelligence notes only similarity, of which contiguity is for it a simple kind and outline."[1]

Thus consciousness, according to Fouillée plays an important rôle in the association of ideas. It

[1] Loc. cit., pp. 382, 383.

reacts on presentations and arranges them harmoniously. While not the primitive force in association, it becomes, by reacting on associations presented to it, the force that dis-associates and analyzes. It has not only the negative power of rejecting ideas, but also the power of increasing by reflection the force of ideas suited to its purposes, thus making one idea a center of attraction, as it were, for other ideas.

In criticism of Fouillée's explanation of the physical mechanism of association, it may be said that it is too simple for an adequate explanation of the incalculable complexity of thought. The kaleidoscopic train of our ideas follows no such simple rules as the traditional laws of association. Fouillée himself, as well as Horwicz and others, has shown the part that feeling plays in the recall of ideas. Hodgson has done the same in showing that association by interest should be added to the other laws, and James gives cogent reason for adding a principle of spontaneous revival.

The importance of the last two principles will be seen by a moment's reflection. Not only does our mental constitution demand an adaptation on the part of ideas to the emotional mood of the moment, and thus our thoughts take a different course as we are sad or happy; but we find interest constantly shunting our thoughts into new channels. This can be readily shown by an illustration similar to those given by Hodgson and James. Suppose I am repeating the oft-quoted lines of Shakespere:

> "Who steals my purse, steals trash," etc.

If none of the words suggest to my mind an idea of peculiar interest, I run through the passage without distraction. But suppose my pocket-book happens to be unusually empty and I know of no easy means of replenishing it. Then, when I come to the

word *purse*, I am likely to hesitate, and my thoughts will probably shoot off upon my financial condition, because that interests me more than Iago's reflections upon the value of a good name and the baseness of calumny.

In this case contiguity works with interest, but contiguity alone would carry my thoughts on without interruption to the end of the passage.

As an example of the spontaneous revival of images, Prof. James mentions the phenomenon sometimes observed before going to sleep when images dance in haphazard fashion before the eyes. This phenomenon occurs most frequently, perhaps, when we are weary; and it may be due, in part, to peripheral excitation. A case of more clearly mental revival seems to occur when we are well rested, and the nerve cells are, perhaps, overcharged with energy. At such a time it often happens that a multitude of images seem to spring up in the background of consciousness and to crowd around our conscious train. Our thought, to use a phrase of Fouillée's, seems to be a process of "intelligent selection." Of course it may be argued that, even in the case of this semi-conscious revival, there are subtle links of subconscious association, but there seems to be an element of spontaneity here, that is lost sight of in the ordinary doctrine of association.

Again the revival of our ideas is determined by the law of habit.[1] Modern studies add weight to the dictum of Bacon that "custom is the principal magistrate of man's life"; for they seem to show that our very thoughts chase each other along habitual paths. Except perhaps with men of genius, it is only in

[1] The relation of memory to habit has long been recognized. For a recent theory explaining memory by habit see Gratacap, Théorie de la Mémoire.

rare moments of special inspiration that our thoughts flash in unwonted channels.[1]

Hodgson has shown how habit works with interest in the spontaneous recall of our ideas.[2] "Two processes," he says, "are constantly going on in redintegration, the one a process of corrosion, melting, decay, and the other a process of renewing, arising, becoming. Unless by an effort of volition, which is here out of the question, (since here only the spontaneous train of thought is under consideration,) no object of representation remains long before consciousness in the same state, but fades, decays, and becomes indistinct. Those parts of the object, however, which possess an interest, that is, those which are attended by a representation of pleasure or pain, resist this tendency to gradual decay of the whole object. . . . . This inequality in the object, some parts, the uninteresting, submitting to decay, others, the interesting, resisting it, when it has continued for a certain time, ends in becoming a new object." This new object consists of the interesting parts of the old object and some sort of an indistinct environment. It is formed under the general law of redintegration, which is, as formulated by Hodgson, "that every object, which has occurred in a variety of combinations, has a tendency to redintegrate, or call back into consciousness, all of them."[3] These interesting parts which persist in consciousness then, as soon as the uninteresting parts of the old object have vanished, are free to unite with any object with

[1] The pathological activities of the mind are instructive upon this point. Paranoia is a case of extreme habituation. The patient has a bad habit of thought. For an excellent illustration of the way the thought of the paranoiac gravitates into one channel see a paper on Insistent and Fixed Ideas, by Edward Cowles, M. D. AM. JOUR. PSYCH. I, 222.

[2] Space and Time, p. 266 seq. Cf. also, Theory of Practice, vol I, p. 367–381.

[3] Space and Time, p. 267.

which they have at any time been combined before. "All the former combinations of these parts may come back into consciousness; one must; but which will? There can be but one answer: That which has been most habitually combined with them before. This new object begins at once to form itself in consciousness, and to group its parts round the part still remaining from the former object; part after part comes out and arranges itself in its old position; but scarcely has the process begun, when the original law of interest begins to operate on this new formation, seizes on the interesting parts and impresses them on the attention to the exclusion of the rest, and the whole process is repeated again with endless variety."[1]

Such is the ceaseless interplay of habit and interest in our spontaneous trains of thought, and our voluntary thought seems still more complicated. It is not necessary to discuss the latter here; but it may be noted that in our ordinary trains of thought the spontaneous and the voluntary alternate, and thus the complexity is still more increased.

In the return of ideas to consciousness then, we find the laws of association modified by other principles, such as congruity with the emotional mood of the moment, habit, interest, spontaneity; and, in addition of course, by the recency and the vividness of the first impression. These are the factors that from the subjective standpoint seem to determine the train of thought in recollection, and it is natural to suppose that the corresponding physical processes are equally complex. The theory of Fouillée can afford but a partial explanation of them; all that can be done seems to be to express in the most gen-

[1] Ibid., pp. 267, 268.

eral terms the cerebral activity upon which recollection depends.

Prof. James has attempted to do this by showing how the laws of association "may follow from certain variations in a fundamental process of activity in the brain."[1] Basing his theory upon the principle that, when two brain processes have occurred simultaneously or in immediate succession, either of them on recurring tends to arouse the other also, and noting the complexity that arises from the fact that each elementary tract has at different times been excited in conjunction with *many* other tracts, and in view of the tension in nerve-tissue and the summation of excitements, he formulates the law of association from the physical standpoint as follows: "*The amount of activity at any given point in the brain-cortex is the sum of the tendencies of all other points to discharge into it, such tendencies being proportionate* (1) *to the number of times the excitement of each other point may have co-existed with that of the point in question;* (2) *to the intensity of such excitements; and* (3) *to the absence of any rival locality or process functionally disconnected with the first point, into which the discharges might be diverted.*"[2]

Another important work of modern psychology has been to show the relation of memory to the time sense. The metaphysical doctrine of the present as a knife-edge has no foundation in experience. Such a present, the mere conterminous of the past and future, may be a metaphysical reality, but it is a psychological nonentity. The only present of which we practically know anything is the immediately intuited past. "If the present thought," says Prof. James, "is of A B C D E F G, the next

[1] See The Association of Ideas, Pop. Sci. Monthly, March, 1880.
[2] Ibid., p. 582.

one will be of B C D E F G H, and the one after that of C D E F G H I,—the lingerings of the past dropping successively away, and the incomings of the future making up the loss. These lingerings of old objects, these incomings of new, are the germs of memory and expectation, the retrospective and the prospective sense of time. They give that continuity to consciousness, without which it could not be called a stream."[1]

The few seconds during which objects are fading from consciousness has been aptly named the "specious present" by Mr. Clay and Prof. James. The most vivid part of it is measured by the experiments on the *Umfang*, or extent of immediate consciousness for successive impressions.[2] Under the most favorable circumstances it may amount to some dozen seconds,[3] with perhaps an obscure background extending still farther into the past. It forms not only "the original paragon and prototype of all conceived times," by reference to which, the past and the future are measured, and without which we should probably have no sense of time, but its echoes of sensations just past make Fechner's memory-after-images possible[4] and constitute what Exner calls our "primary memory,"[5] and Richet our "elementary memory."[6]

That a stimulus leaves behind it an excitation that only gradually passes away is proved first by such facts as the possibility of perceiving an electric spark that lasts only a hundred-thousandth of a second,[7] the well-known fact of the summation of

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[1] Wm. James: The Perception of Time, Journal of Speculative Philosophy, XX, pp. 375, 376.
[2] See Wundt: op. cit., Bd. II, p. 213 seq.
[3] Cf. Dietze's experiments, Philos. Studien, II, p. 362.
[4] See my 2d article, AM. JOUR. OF PSYCH., II, 253, 254.
[5] See Hermann's Handbuch d. Physiol., Bd. II, Thl. II, p. 281.
[6] Loc. cit., p. 562.
[7] Cf. Richet, loc. cit., pp. 561, 562.

stimuli, and the phenomena of sensorial after-images; secondly, by the vividness of the memory-after-image, which renders possible an accurate comparison of impressions. To quote again from Prof. James's admirable paper: "We may read off peculiarities in an after-image left by an object on the eye which we failed to note in the original. We may 'hark back' and take in the meaning of a sound several seconds after it has ceased. . . . . With the feeling of the present thing there must at all times mingle the fading echo of all those other things which the previous few seconds have supplied. Or, to state it in neural terms, there is at every moment a cumulation of brain processes overlapping each other, of which the fainter ones are the dying phases of processes which, but shortly previous, were active in a maximal degree. *The amount of the overlapping determines the feeling of the duration occupied.*"[1]

Many of the older writers were wont to say: "Our notion of duration, as well as our belief in it, is got by the faculty of memory."[2] This is true in a certain sense. Memory and the feeling of duration are inseparably connected; both alike seem to depend on the decaying processes — not only sensations but acts of attention and of thought as well — that fill the immediately intuited past. All that is necessary for the elementary form of either is the simultaneous presence of an active and a fading image. If a man were instantaneously created "with a brain in which were processes just like the 'fading' ones of an ordinary brain, the first real stimulus after creation would set up a process additional to these. The processes would overlap;

[1] Loc. cit., p. 401.
[2] Reid, Essays on the Intellectual Powers of man, Essay III.

and the new-created man would unquestionably have the feeling, at the very primal instant of his life, of having been in existence already some little space of time."[1]

The mysteries of memory and the sense of time are by no means solved, but the discussion has shifted from metaphysical to psycho-physic ground; and they who would study memory and the feeling of duration in their elementary form, must study the *Umfang* of consciousness.

Finally the feeling of personality depends upon memory. Or rather, with John Stuart Mill, we may say: "The phenomenon of Self and that of memory, are two sides of the same fact, or two different modes of viewing the same fact. We may, as psychologists, set out from either of them, and refer the other to it. We may in treating of memory, say that it is the idea of a past sensation associated with the idea of myself as having it. Or we may say, in treating of Identity, that the meaning of Self is the memory of certain past sensations."[2] Neither of these facts can be explained; but probably both alike depend primarily upon the persistence of sensations and the gradual fading of images. But neither would be complete without the revival of impressions. With the present perceptions mingle the memories of similar perceptions. If the object before us is a tree, the memories of other trees arise along with the sensations from it. With the impressions A, B, C, D, come the memories a_1, a_2, a_3, b_1, b_2, b_3, and so on. "Our consciousness is then," as Richet says, "always in the presence of a certain limited number of old images, that are always nearly the same, and

[1] James, loc. cit., p. 406.
[2] Note in his edition of James Mill's Analysis, Vol. II, p. 174.

these images being related to the same *me*, are the basis of the individual's personality — a personality that is rendered stable enough by the community of the images." [1]

Recent pathological studies have furnished some remarkable evidence for the view that memory and the feeling of personality are "two sides of the same fact." Whenever in disease, or somnambulism, or by hypnotic experimentation the old memory-images are shut out of consciousness, a new personality results.[2] If the conscious life is broken up into several memories, as many new personalities are improvised. It is not necessary to recount here the well-known hypnotic experiments at Paris, Nancy, and elsewhere; nor does space permit a discussion of the question whether the results of such studies affect the old notion of a transcendental self; but it is clear that the integrity of the personality depends upon the unity of memory, and anything that seriously affects the latter is likely to affect the former also.

II.—*Experimental Studies.*

The most important attempt to apply the methods of experimental psychology to the study of memory was made a few years ago by Dr. Ebbinghaus of Berlin. What he modestly calls the provisional results of his investigation are reported in a valuable monograph.[3] He experimented only upon himself; and, as he admits, his results have directly only an individual value. Nevertheless, his long and laborious series of experiments are so valuable both for their immediate results and as showing the advantages and limitations of psycho-physic methods in

[1] Loc. cit., p. 589.
[2] For examples cf. Ribot: Maladies de la personnalité.
[3] Ueber das Gedächtnis, Leipzig, 1885.

the study of memory, that a brief abstract of his work will be given here.

As the basis of his study Ebbinghaus lays down the principle that psychic states of every kind are not annihilated when they vanish from consciousness, but their effects persist unconsciously. Three classes of facts prove the persistence of states of consciousness apparently lost: 1st, in many cases we can *voluntarily reproduce* the lost states; 2d, what has once been present to consciousness, may return, sometimes after years, without any special effort, i. e., is *involuntarily reproduced;* 3d, vanished states of consciousness give evidence of their continued existence by rendering similar thought processes easier.

Great difficulties are encountered in experimenting upon memory. The conditions upon which a good memory depends vary with different individuals, and with the same individual from morning to evening, from youth to age. Moreover, much depends upon the content of the matter to be remembered. It is hard, for example, to remember forms and colors; and past states of feeling can be reproduced with difficulty. In the first place then, how shall the multitude of determining conditions be kept constant? In the second place, what numerical measure shall be found for anything so complex and evanescent as psychic processes? Ebbinghaus attempted to overcome these difficulties as follows.

From the simple consonants and the eleven German vowels and diphthongs all the syllables possible of a definite kind were formed, placing a vowel between two consonants. These nonsense syllables, about 2300 in number, were shuffled, and then, as they were drawn by lot, formed into series of different lengths. Several of these series were used at each experiment. The syllables were read

aloud repeatedly until it was just possible to recall them voluntarily. This end was considered attained "when a series, the first syllable being given, could be repeated by heart for the first time without hesitation, at a given rate of speed, and with the consciousness of perfectness."

This nonsense material has the following advantages. In the first place, it is relatively simple and homogeneous. While prose or poetry constantly changes in character, and varies in interest with the individual apperception, among more than 2,000 of these syllables, scarcely a dozen make any sense, and in the process of learning, the thought of this meaning is seldom aroused. The difference, however, between sense and nonsense syllables is not as great in this respect as would be expected *a priori*. For the varying content of the former is offset in case of the latter by predilections for different letters and syllable-combinations due to the influence of the mother-tongue. The advantage of nonsense material is considerable, however, in two other respects, namely, that it affords an inexhaustible amount of new combinations of similar character, and that it offers means for convenient quantitative variation.

In learning these nonsense syllables careful attention was given to details of method. A series was read through completely from beginning to end; then the first syllable was read, and the attempt was made to repeat the series. At the first hesitation the remainder was read, and then the repetition was begun anew. Reading and repetition were as nearly as possible at a uniform rate, namely, 150 syllables a minute, regulated by the ticking of a watch. The voice was kept at a uniform pitch with the accent upon every third or fourth syllable. After learning a series a pause of 15 seconds was

made to note results, and then the next series was attempted. Disturbances were removed, the attention concentrated, and the aim was to learn the series as quickly as possible. No attempt was made to bind the syllables by fancied relations. And, finally, care was taken especially that all the conditions of life during the period of experimentation should be as nearly uniform as possible. In the case of any great change in the mode of life, Ebbinghaus deferred the experiments, and a period of practice preceded their renewal. Similar experiments were performed at the same hour of the day, and care was taken that the activity preceding the experiments should be of the same character.

Two sources of error are noted by Ebbinghaus. First there are flickerings of the attention, or the like, that cause variations in the ability to repeat a series perfectly. These variations, however, may balance in a large group of series. The other possible source of error is a very dangerous one. It is the unconscious influence of theories and opinions of the experimenter. Even if he tries to avoid prejudice, that very effort is liable to vitiate the results. But, as the aim of the experiments was to obtain relative rather than absolute results, Ebbinghaus concluded that there was no reason to be very suspicious on this account.

The experiments were made at two periods, namely: 1879–80, and 1883–84, each period extending over a full year. Testing experiments of a similar kind preceded each period, so that in the results given the effect of growing practice may be disregarded. In the later period all the experiments were made between the hours of 1 and 3 p. m.; in the earlier one. they were divided unequally into three groups, performed between 10 and 11 a. m., 11 and 12 a. m., and 6 and 8 p. m.[1] By taking

[1] The latter groups will be designated A, B, and C respectively.

groups of an equal number of successive series separated by considerable intervals of time, the variations due to fatigue, fluctuation of the attention, and the like, were largely balanced. This detailed account of the method is necessary to a just estimation of the results to be detailed.

The first point is the dependence of the rapidity of learning a series upon the length of the same. It is well known that it takes a proportionately longer time to acquire a long series of ideas than a short series. Ebbinghaus's experiments corroborate this. He found that he could generally repeat a series of seven syllables after once reading them. Sometimes eight syllables could be repeated perfectly after one reading, six almost always; but about seventeen repetitions were required for a series of twelve, and nearly thirty for one of sixteen syllables. Of course these results have only an individual value; but it is noteworthy that for this experimenter they long remained constant.

By another group of experiments the process of learning poetry was compared with that of learning nonsense syllables. Stanzas from Byron's Don Juan were learned, and Ebbinghaus estimated that only about one-tenth the number of repetitions was required for learning material thus connected by the bonds of sense and rhythm.

The next important problem was the relation of retention to the number of repetitions. When a series once learned is forgotten, the strength of the impression persisting unconsciously can always be measured indirectly by the saving of time required for re-learning the series. To determine the effect of many repetitions upon the readiness with which a series can be reproduced, Ebbinghaus made seventy double experiments of which each experiment consisted in reading or repeating six sixteen-

syllable series a definite number of times, and then, twenty-four hours later, re-learning them. The experiments were divided into seven groups of ten experiments each, the number of repetitions in the seven groups being respectively 8, 16, 24, 32, 42, 53 and 64. The results are embodied in the following table,[1] the numbers being the number of seconds required for the re-learning.

	After 8 repetitions.	After 16 repetitions.	After 24 repetitions.	After 32 repetitions.	After 43 repetitions.	After 53 repetitions.	After 64 repetitions.
	1171	998	1013	736	708	615	530
	1070	795	853	764	579	579	483
	1204	936	854	863	734	601	499
	1180	1124	908	850	660	561	464
	1246	1168	1004	892	738	618	412
	1113	1160	1068	868	713	582	419
	1283	1189	979	913	649	572	417
	1141	1186	966	858	634	516	397
	1127	1164	1076	914	788	550	391
	1139	1059	1033	975	763	660	524
Mean.	1167	1078	975	863	697	585	454
Probable error.	±14	±28	±17	±15	±14	±9	±11

The average time for the first learning of six series of sixteen syllables each, as estimated from fifty-three experiments was 1270 seconds, with the small probable error of $\pm$ 7. Comparing with this the times required for re-learning as given in the table, Ebbinghaus estimates that, on an average, in re-learning there is a saving of one third due to the persisting after-effect of the repetitions. In other words for every three repetitions to-day a person is saved about one repetition twenty-four hours afterwards.

The same experiments show the relation of recognition to the number of repetitions. When the series were impressed upon the mind with only eight

[1] Op. cit. p. 75.

or sixteen repetitions they were not recognized on the following day. When, however, there were fifty-three or sixty-four repetitions, they generally were recognized.

Further experiments indicated that this saving does not continue in the same ratio when the number of repetitions is increased above sixty-four. If for each repetition one third its value is saved in learning the series the following day, then it would seem that, if a series is repeated three times as much as is necessary to learn it, it should be possible to recall the series the next day. But even with four times the number of repetitions, this proved impossible.

A still more important group of experiments showed the relation of retention to time. 163 double experiments were performed. Each experiment consisted in learning and re-learning eight thirteen-syllable series. The re-learning was done at seven different intervals, approximately as follows: one-third of an hour, one hour, nine hours, one day, two days, six days, and thirty-one days. The learning was continued until it was possible to repeat a series twice without error. The time was measured from the completion of the learning. The results appear in the following table.

Time interval.	No. of seconds required for first learning.	No. of seconds required for re-learning.	Gain in seconds.	Gain in per cent	Probable error.
19 min.	1081	498	583	58.2	1
63 "	1106	647*k*	459	44.2	1
525 "	1132	752*k*	380	35.8	1
1 day A	1109	756	353	33.8	2
" B	853	599	254	32.6	2.2
" C	1184	803	381	34.6	2.3
2 days A	1154	854	300	27.2	2.3
" B	891	647	244	28.2	3.5
" C	1245	917	328	28.1	1.8
6 days A	1090	834	260	25.2	1.9
" B	872	652	220	26.1	4
" C	1306	989	317	24.9	1.6
31 days A	1115	892	223	21.2	1.3
" B	879	710	169	20.8	1.4
" C	1261	1007	254	21.1	2.7

Examination of the table[1] shows that the process of forgetting is rapid at first and then slower. After the interval of an hour so much has been forgotten that more than half the original work must be done again before the series can be reproduced. After eight hours almost two thirds of the original work is necessary. But from that point the process of forgetting proceeds more slowly. After twenty-four hours the impression still retains about a third of its original strength; after six days, a quarter; after a month, still a full fifth. It is noteworthy

[1] Op. cit. p. 94 seq. Ebbinghaus gives tables for each group of experiments. I have collected in this table the *mean* results only.

A, B and C designate experiments performed at the hours of 10—11 A. M., 11—12 A. M. and 6—8 P. M. respectively. In estimating the per cent. of saving, the time for the final two perfect repetitions, i. e., 85 sec., was deducted from the time for learning as given above. The figures followed by *k* in the 2d and 3d groups give the corrected times for re-learning. As there was an unavoidable variation in the conditions due to the increased fatigue at the time of re-learning in the later hours of the day, a number of seconds were deducted from the original results.

that, while the impression made by nonsense syllables is so evanescent that a series once perfectly learned is forgotten after an interval of twenty minutes, a residuum of some sort persists for a long time, so that even after a month the same series can be re-learned in four fifths of the time originally required. A general statement of the results is as follows: The ratio of what is retained to what is forgotten is inversely as the logarithm of the time[1].

Another group of experiments showed the interesting result, that a long series is more strongly impressed upon the mind by once learning than a short series. For example, a series of thirty-six syllables is nearly twice as firmly impressed as one of twelve syllables.

Further experiments showed that when there are a large number of repetitions a suitable distribution of them over a certain interval of time is more advantageous than the accumulation of them all at once. Thus in learning nine twelve-syllable series the same impression was made by thirty-eight repetitions distributed over four successive days as by sixty-eight consecutive repetitions.

Ebbinghaus experimented also upon the association of ideas by contiguity. This law of association is generally formulated as follows: Presentations once aroused in consciousness simultaneously, or in immediate succession, reproduce each other, and more easily in the direction of the original succession, and with greater certainty the oftener they have been together. But one point may be mooted. When a series *a b c d e f g* has been learned, does *a*

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[1] Ebbinghaus's formula was:

$$\frac{\textit{das Behaltene}}{\textit{das Vergessene}} = \frac{K}{(\log. t)c},$$

in which K and c are individual constants.

recall *b*, and *b* recall *c*, and so on, each member of the series being connected only with the succeeding member; or does *a* recall *b*, and tend also to recall *c*, *d*, and the rest, each member being associated not only with the succeeding member but also, though in a less degree, with the whole series? The Herbartians maintain that the latter is true for so many members of the series as have once been together above the threshold of consciousness.[1] To bring this problem to a scientific test, Ebbinghaus performed the group of experiments next to be described.

When a series of nonsense syllables is learned by heart, and re-learned in the same order on the next day, there is, as we have seen, a saving of about one third the original work. This one third measures the strength of the association from one member of the series to the succeeding member. Suppose the syllables are re-learned in a different order; what will be the result? Fifty-five double experiments were performed to determine this point. Six sixteen-syllable series were employed in each experiment. If the different series are represented by Roman, the syllables by Arabic numerals, the series employed would be as follows: $I_1\ I_2\ I_3 \ldots\ldots I_{15}\ I_{16}$, $II_1\ II_2\ II_3 \ldots\ldots II_{15}\ II_{16}$, and so on. New series were formed by Ebbinghaus from the alternate syllables as follows: $I_1\ I_3\ I_5 \ldots\ldots I_{15}\ I_2\ I_4\ I_6 \ldots\ldots I_{16}$. Except in the middle, where there is a break, each syllable of the transformed series was separated from its present neighbor by an intervening member in the original order. If these intervening members were essential hindrances to the bonds of association, then the new series is as good as

[1] See Herbart's Lehrb. z. Psychol., §29; also my 2d article, AMERICAN JOURNAL of PSYCHOLOGY, II, 248.

unknown; and one can expect no saving of labor in learning the transformed series. If, on the contrary, the bonds of association extend not only from one member to the succeeding members, but also over intervening members to more distant syllables, then there will be a certain predisposition to the new series. The succeeding syllables of the series will still be united to each other with a certain strength, and less work should be required to re-learn them than to learn an entirely new series. The amount of this saving will be a measure for the strength of association over intervening members. If series are derived by skipping two, three or more syllables, the same considerations apply to them. Ebbinghaus derived similar series by skipping two, three, and seven syllables. Skipping seven, the derived group of series was as follows: I_1 I_9 II_1 II_9 III_1 III_9 IV_1 IV_9 V_1 V_9 VI_1 VI_9, I_2 I_{10} II_2 II_{10}, and so on, the last series being V_7 V_{15} VI_7 VI_{15} I_8 I_{16} II_8 II_{16} III_8 III_{16} IV_8 IV_{16} V_8 V_{16} VI_8 VI_{16}. By examining series derived in this manner, it will be seen that the breaks increase with the increasing number of syllables skipped. Where the different series are mixed, there is no association except between syllables that originally were in the same series. Hence, the derived series suffer from a certain unavoidable inequality.

Each experiment consisted in learning the six original series; and then after twenty-four hours re-learning them in the derived order. The result showed a remarkable saving of time in re-learning the series. Skipping one, two, three, and seven syllables, the mean saving in learning the derived series was 152 sec., 94 sec., 78 sec., and 42 sec., the mean time for learning the original series being 1266 sec.

The objection may be raised that the syllables

are impressed upon the mind by the first learning, not only in their definite order, but also as separate members. With reference to this objection, Ebbinghaus experimented, retaining the initial and final syllables of the six original series employed in an experiment, and using chance combinations of the other eighty-four syllables. In the case of series formed thus by mere permutation, the mean saving was only .12 sec., the mean time for learning the original series being 1261 sec.

Again, it may be objected that the saving of time in re-learning was due to an unconscious desire to bring about this result. In case of the first two or three re-arrangements, the saving is so considerable that it could hardly be due to any such influence. Then too, the regular decrease in the numbers would hardly occur if that were the case. Nevertheless, to test this point, thirty experiments were made in the following manner:

Six sixteen-syllable series were written upon a sheet of paper. On the back side of the sheet six derived series were written. Six sheets were thus prepared for each of the five transformations described above, making 30 in all. The sheets of each group could not be distinguished from each other on the front side; and they were shuffled and laid aside until the memory of what individual syllables occurred in a given derived series had vanished. Then the series on the front side of one sheet of paper were learned; and after twenty-four hours those on the back side of the same sheet. The time necessary for learning the separate series was noted, but the results were not reckoned up until the whole thirty sheets had been finished. Then these experiments were found to corroborate the results of the earlier ones. Skipping one, two, three, and seven syllables, the saving in learning the derived series was 110

sec., 79 sec., 64 sec., and 40 sec. respectively, the mean time for learning the original series, as before, being about 1266 sec. Deriving the new series by permutation, the re-learning required a mean application of 5 sec. more than the original learning, the mean time for the latter being 1261 sec.

Putting the two sets of results together to obtain a possible balance of disturbing influences, Ebbinghaus estimates the results of the eighty-five experiments in percents as follows.

Strength of association, as measured by the saving of time in re-learning six sixteen-syllable series:

Between contiguous members	33.3 per cent.
Skipping one syllable	10.8 "
" two syllables	7.0 "
" three "	5.8 "
" seven "	3.3 "
With permutation	0.5 "

Ebbinghaus emphasizes the interesting fact that, when the derived series were formed by permutation of the syllables, retaining only the initial and the final members of a series, no saving was shown. The new series formed of the identical syllables originally used could be learned in scarcely less time than an entirely new series. But a series derived by skipping seven syllables could be learned in noticeably less time than an entirely new one. As seven was the number of syllables that Ebbinghaus could learn by a single repetition, it might seem that the associations described were due to the fact that seven syllables were embraced in consciousness at once. But seven syllables were skipped, thus showing association between the first and the ninth members of a series. Moreover the numbers representing the strength of the association were so great and the gradation of them of such a nature, that it seems probable that the bonds of association extend even to more distant members of the series. If

there are associations, argues Ebbinghaus, extending to more syllables than can be embraced in one act of consciousness, then these associations cannot be explained simply on the theory that presentations included in one act of consciousness tend to arouse each other.[1]

He also experimented to determine whether there are bonds of association extending backward. He took six sixteen-syllable series and made ten experiments with series derived by mere inversion of the syllables, and four with series derived by inversion and skipping one syllable. In the first case the saving in re-learning amounted to 12.4 per cent.; in the last to 5 per cent.

Further experiments were made to determine the effect of many repetitions. The result showed that while an increased number of repetitions strengthens all the associations, the stronger bonds between neighboring members of a series are strengthened more rapidly than the weaker ones between remote members.

The general results of this whole study of association are summarized as follows:

"In the impressing and strengthening of a series of ideas by numerous repetitions of the same, inner bonds or associations are formed between all the individual members of the series. Every member of the series preserves a certain tendency in case of its return into consciousness to bring the other members with it.

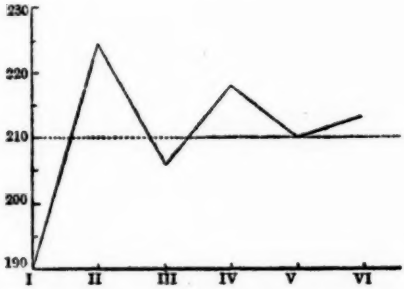
"These bonds or tendencies are of different degrees of strength in several respects. For the remote members of the original series they are weaker than

[1] Compare Wundt's experiments on the *Umfang* or extent of immediate consciousness for successive impressions, Physiol. Psych., 3te Aufl., Bd., II., p. 213 seq.

for neighboring members, for definite distances backward, weaker than for the same distances forward. In case of increasing number of repetitions the strength of all the bonds increases. But the already stronger bonds between the neighboring members are by this means considerably more quickly strengthened than the weaker bonds between more distant members. Therefore the more the number of repetitions increases, so much the stronger become absolutely and relatively the bonds of the members immediately succeeding each other, and so much the more exclusive and predominant becomes the tendency of each member when it returns into consciousness to bring after it that one which in the repetition always immediately succeeded it."[1]

From the study of his results Ebbinghaus found indication of a remarkable rhythm of the attention. A series learned in a proportionately short time was followed as a rule by a series learned in a relatively long time. "There seems to be," he says, "a kind of periodic oscillation of the mental susceptibility or of the attention, in which the increasing fatigue appears in variations about a gradually shifting middle position."[2] Nearly all the experiments showed this oscillation. The results of a typical group of experiments were as follows: In eighty-four experiments with six sixteen-syllable series the mean time for learning the 1st series was 191 sec.; for the 2d, 224 sec.; for the 3d, 206 sec.; for the 4th, 218 sec.; for the 5th, 210 sec.; for the 6th, 213 sec. Representing these results diagrammatically with the number of seconds marked off on the ordinates and that of the series on the abscissas, the curve will be the following:[3]

[1] pp. 160, 161.
[2] p. 60.
[3] p. 58.



In experimenting with derived series, however, the phase of the rhythm was changed, and the even numbered series were learned quicker than the odd.[1]

Such in brief outline are the results of Ebbinghaus's laborious and painstaking experiments. Fully to appreciate his scientific rigor and subtle analysis one must read his own work. Some of his experiments have revealed wholly new facts, and others have added a most needful accuracy to what was indefinite. To the latter class belong the experiments on the unconscious persistence of impressions. Thoughtful observers have believed that things forgotten were not wholly lost; the Herbartians have imagined a mysterious subconscious glimmer in the soul; Richet a trace in the brain substance; Luys and others a persisting vibration; Maudsley a

[1] Ebbinghaus explains this by an ingenious hypothesis of indirect, unconscious association. When the first derived series, made up of syllables 1, 3, 5, etc., is learned, there is a tendency for these syllables to bring with them also syllables 2, 4, 6, etc., though too weak a one to bring them into consciousness — merely an inner excitation, such as occurs when one tries in vain to recall a forgotten name. This, however, strengthens the sub-conscious bonds of the even numbered syllables, and if the derived series which they form is learned *after* that composed of the odd syllables, it is learned more easily. "Inner bonds are formed between successive syllables unconsciously aroused, as between successive conscious syllables, only of course of less strength." — (p. 168.)

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"neurotic pattern"; James a "path of association"; Ribot and Wundt a "functional disposition"; and some of the intellectualists have given up the attempt to name it altogether. But whatever it is, the experiments of Ebbinghaus bring it under the yoke of numerical determination. The curve of forgetting for long periods of time has also been determined; and the work upon association has put an experimental foundation under the Herbartian doctrine that each member of a series is linked not only to the following member, but also in lessening degrees to those more remote.

All psychophysic experiments, especially those requiring comparison and those upon the time-sense and the like, involve memory. The experiments of Wundt[1] and his pupils[2] upon the *Umfang*, or extent of immediate consciousness of successive impressions, concern directly what Richet calls the "elementary memory".[3] But to Ebbinghaus belongs the credit of first performing a series of scientific experiments with special reference to the phenomena of memory. His success in testing, by an indirect method, the association of ideas in a series, suggests the possibility of extending the experimental method to apparently inaccessible psychological problems. Useful as his method proved, it was not, however, the simplest one. The learning of nonsense syllables is a complicated psychological process; and while such syllables are free from the associations that cluster about significant material, nevertheless, some syllables are easier than others on account of the eye and

[1] Op. cit., p. 213 seq.

[2] Especially Dietze, Untersuchungen über den Umfang des Bewusstseins bei regelmässig aufeinander folgenden Schalleindrücken, Phil. Studien, II., 362—394. Also Cattell, Ueber die Trägheit der Netzhaut und des Sehcentrums, Phil. Studien, III., 94—127; and Hall and Jastrow, Studies in Rhythm, Mind, XI., 55—62.

[3] Loc. cit., p. 568.

ear memories and the muscle memory of the vocal organs. A simpler method is to test the power of recognizing a sense-impression when repeated. It is a well-known fact that such recognition is easier than ordinary recollection without repetition of the external impression. Thus we distinguish many more shades of color than we can picture in imagination; and we generally can understand more words of a language than we can use. H. K. Wolfe adopted this simpler method in investigating the memory for tones. He experimented as follows.[1]

Nearly 300 vibrating metal tongues, giving notes through five octaves, were used. A tone was given, and after an interval determined upon beforehand, either the same tone was repeated or a higher or lower one was sounded. The listener wrote one of two answers—"same" (*gleich*) or "different" (*verschieden*). If the answer was the latter, a further judgment was made whether the second tone was higher or lower than the first, or the subject could answer "doubtful." The difference of tone amounted to 4, 8, or 12 vibrations a second, and was kept constant during a group of experiments.

The main point of the research was to determine the influence upon memory of the time-interval between the impressions to be compared. This was varied from one second to 30, 60, or 120 seconds. The greatest accuracy was found when the interval was about two seconds. Three reasons, he thinks, may account for the failure to judge correctly when the interval is less than this: 1st, as we are apt to overestimate short intervals of time the attention may not be focused when the second sound is heard; 2nd, there may not be time, when the interval is very short, fully to perceive the two impressions; or 3d, it is possible

[1] See Ueber das Tongedächtniss, Philos. Studien, III., 4.

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that an excitement of the sense-organ persisting from the first impression intensifies the second, and hence the latter is incorrectly perceived.

As the time was increased beyond two seconds, his experiments showed that the curve of memory fell pretty regularly until the interval reached a point somewhere between 10 and 20 sec. Here with each subject Wolfe found a point where forgetting seemed to be retarded or to cease altogether. In case of W. and L., the two subjects with whom the largest number of experiments were made, this swell in the memory-curve occurred, with the former, at an interval of about 15 sec., with the latter at an interval between 20 sec. and 25 sec. Beyond this point the curve falls off still more rapidly with increasing time. But some of the subjects showed traces of possibly a second swell in the curve at an interval from 30 sec. to 50 sec. While smaller irregularities in the curve disappeared with a larger number of experiments, over 4,000 experiments in case of L., and 12,000 in case of W., showed the first retardation point tolerably clearly.

The results in case of these two subjects with whom the largest number of experiments were performed, may be expressed approximately, as Wolfe estimates, in the following simple mathematical formula: The ratio of the right and wrong cases is inversely proportional to the logarithms of the time-intervals[1].

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[1] Letting r = the correct judgments, f = the errors, and K and c = individual constants; the formula is

$$\frac{r}{f} = \frac{K}{\text{Log. } t} + c,$$

or

$$r = \frac{Kf}{\text{Log. } t} + cf.$$

Ebbinghaus's formula was similar. (See *supra*). It is an interesting question whether this law would hold with other xperimenters and for all the senses. Dr. Jastrow found in testing the eye and hand

Thus Wolfe's experiments with short intervals corroborate those of Ebbinghaus with longer periods.

The results of Ebbinghaus's study, as has been said, have primarily only an individual value. So far as I am aware, there is no record of any adequate attempt to test these results with many different individuals. A pupil of Kraepelin, however, Dr. Oehrn, has made a suggestive study of individual psychology[1]; and in reference to memory he used substantially the method of Ebbinghaus, although in part substituting number series for nonsense syllables. But his experiments thus far reported have given no satisfactory results as far as memory is concerned, except to corroborate the testimony of others, that in the performance of any mental function there are great individual variations, and to show that in such experiments the mean variation is a direct measure of the share of the attention in the process. Thus, in learning the tedious nonsense syllables, the average mean variation for his ten subjects was nearly six times as large as in the more interesting process of adding[2].

Upon one point, however, the experiments of Ebbinghaus have been supplemented by a research

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memories for measurements, that "in both cases the memory is extremely accurate and is almost as faultless after the lapse of a few days as of a few minutes." See The Perception of Space by Disparate Senses, Mind, XI., p. 552. The same article contains experiments upon the relative accuracy of the eye-arm-and-hand memories for space perceptions.

Beaunis has reported the provisional results of somewhat similar experiments upon muscular sensations. He found three phrases in the vanishing of muscular memory when tested by the power to reproduce the extent and direction of movements: 1st, *the phase of conscious memory;* 2d, *the phase of unconscious memory,* where conscious memory has vanished, but it is still possible to reproduce the given movements; 3d, *the phase of total obliviscence.* See Rev. Philos., XXV, pp. 369—574.

[1] Experimentelle Studien zur Individualpsychologie. Inaug. Dis. Dorpat, 1889.

[2] See review of his paper in the experimental section of this number. ED.

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upon many individuals. Mr. Jacobs and Mrs. Bryant have tested the powers of school children in reproducing numerals and letters after once hearing them[1]. Their most important experiments were performed with the boys of the Jews Free School and the girls of the North London Collegiate School. Numerals (omitting *seven*) and letters (omitting *w*) were employed as more uniform in pronunciation and rhythm than nonsense syllables. The letters or numerals were dictated *once* in a monotonous tone at the rate of 120 sounds a minute, and the pupils repeated orally or wrote as accurately as they could the series given. The following are the most important results of this interesting research.

In the first place, the mental "span"—as the experimenters called the ability measured by the maximum number of syllables that could be repeated after once hearing them—showed a decided increase with the age of the pupils. Boys of 11 years could repeat in this way 6.5 numerals and 5.5 letters; of 12 years, 6.8 numerals and 5.7 letters; of 13 years, 8.8 numerals and 7.9 letters. The following table shows the results of a large number of observations at the girls' school:—

Age,	8	9	10	11	12	13	14	15	16	17	18	19
Number of subjects,	8	13	19	36	41	42	42	72	66	50	30	14
Average number of numerals,	6.6	6.7	6.8	7.2	7.4	7.3	7.3	7.7	8	8	8.6	8.6
Average number of letters,	6	7	6.6	4.6	6.5	6.7	6.7	7.4	7.9	7.3	8.2	7.9

Again the relation of this mental span to the pupil's rank in the class was clearly marked. "As a rule high span went with high place in the form." In a class of twelve-year-old boys the average span for numerals was 9.1 for the first ten, 8.3 for the next

[1] Mind, XII, pp. 75 seq.

and 7.9 for the lowest ten. In another class of thirty boys of the same age, the first ten could grasp 7.6 numerals, the second ten 7.1, and the lowest only 6.3.

Similar results were found at the girls' school. The first half of a class usually showed a higher span than the second half.[1]

Mr. Galton, Prof. Bain, and Mr. Sully made similar observations on the memory-power of idiots. Nine of the best girls among the inmates of one asylum were tested with numerals. One could recollect 2 figures, one 3, three 4, and the remainder 5. And at another asylum where the inmates were of higher mental capacity, some were found with a higher span.

Oliver Wendell Holmes experimented in this manner some years ago[2], and was surprised to find how frequently mistakes are made in repeating between seven and ten figures or letters. An individual may sometimes be found, he says, who can repeat a larger number, and he refers to the famous cases on record where persons have repeated sixty or more unconnected words after once hearing them[3]. But the latter, apart from a few anomalous cases, are hardly to be considered here; for usually the words are repeated slowly and the hearer links them by mnemonic devices.

The amount of this memory-span evidently bears some relation to the mental capacity; and Dr. Holmes has suggested that in such experiments we have "a very simple mental dynamometer which may yet find its place in education."[4] Certainly this

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[1] Dr. Cattell found that a distinct shortening of the time required for the mental process of association accompanies growth and education. See Mind, XIV, p. 234.

[2] Op. cit., p. 32.

[3] For an account of cases of remarkable memory see Huber, op. cit., p. 36 seq; also Jour. of Spec. Phil., V, pp. 6–26.

[4] Loc. cit, p. 33. Mr. Jacobs makes the same suggestion. See loc. cit., also Mind XI, p 54.

test or some other direct observation of memory should be used as a test for cerebral fatigue; for experiments show that memory is very sensitive to changes in physical condition.[1]

Much experimental study has been devoted to the association of ideas. The most extended researches have been made by Trautscholdt[2] and Cattell.[3] The former sought to determine the time required to perform an ordinary act of association, and classified the results of a large number of experiments according to the nature of the association. The latter has extended the research to various forms of association, and has obtained further statistics of its nature. These important studies are too well-known to require extended comment. But as yet, valuable as they are, they have thrown no great light upon memory; for the experiments have been comparatively few in each form of association investigated; and in such experiments, the mental processes measured are inevitably complicated, voluntary recollection being mixed with spontaneous, and both with acts of judging.[4] They are, however, suggestive; they illustrate the complexity of the process of association, and show the important rôle of obscure processes in reproduction.

The average time for the mental process of association was found by Trautscholdt to be 0.727 sec. It is noteworthy that this time is approximately the interval that numerous observers have found can be most accurately reproduced. From this fact

[1] Cp. one of Galton's tests: La fatigue mentale, Revue Scientifique, Jan. 26, 1889, p. 101.

[2] Philosophische Studien, I, p. 213 seq.

[3] Reported in Mind, XII, pp. 68—74 and XIV, pp. 230–250. Cf. also his earlier experiments on the time required for verbal operations; Mind, XI, pp. 63, 220,, 377, 524.

[4] Cp., the remarks by Cattell's subjects, Mind, Vol. XIV, p. 244 seq. See also Dr. Münsterberg's criticism of Trautscholdt's experiments, Beiträge z. experimentellen Psychologie, Heft 1, p. 87 seq.

Wundt concludes "that a rate of about three fourths of a second is that at which processes of association are most easily performed, and which therefore, in the reproduction of objective time-intervals we involuntarily seek to equal by shortening longer periods and lengthening shorter ones.[1] Remarkably enough this time nearly corresponds to the time required for the swinging of the legs in rapid walking. It does not appear improbable that both the psychic constants of the average speed of reproduction and of the most accurate time estimate have been formed under the influence of the most habitual bodily movements, which have also determined our inclination to divide greater periods of time rhythmically.[2]"

Dr. Alfred Lehmann, of Kopenhagen, has studied another aspect of the subject,[3] and attempted to prove that all association can be explained by the single law of contiguity. Like Wolfe, he employed the method of right and wrong cases, and tested the power of recognition; but he experimented with sensations of color, using different shades of gray produced by means of rotating disks, partly black and partly white. In part his experiments supplement the work of Wolfe, and tend to show that the same laws apply to recognition by sight as by hearing; for, varying the time-interval, from 5 sec. to 120 sec., five series of 30 experiments each showed that as time increased, the number of correct judgments decreased with one observer from 30 to 17; with the other from 21 to 17. The experiments give some support to the author's thesis in regard to the laws of association.

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[1] Cp., however, the experiments reported by Stevens, Mind XI, p. 393 seq.

[2] Op. cit., II, p. 286. But see also Galton, who used a different method and found the rate of association to be about 50 ideas a minute; Enquiries into Human Faculty, p. 188 seq.

[3] Ueber Wiedererkennen, Philos. Studien, V, 96—150.

Among experimental methods should also be mentioned hypnotic tests of memory. Perhaps the most important result of these studies has been to demonstrate the existence and importance of what Bernheim calls "latent memories,"[1] which account for the performance of suggested acts after long intervals of time. Space is not left for us to consider here the relation of memory to the hypnotic sleep; but an account of the main facts may be found in the works of Delbœuf,[2] Janet,[3] Bernheim[4] and others, and in the convenient little résumé by Dr. Dichas.[5]

III.—*Conclusion.*

In this brief study of memory from the earliest mythology to recent experimental researches, representative philosophers have been chosen. If the more idealistic and transcendental theories may sometimes seem to have been slighted, it is because an adequate presentation would lead too far into metaphysics rather than lack of appreciation of them. The aim has been to avoid the wider region of philosophical inquiry, upon the borders of which we have repeatedly found ourselves. Still, limiting our inquiry, a multitude of problems arise. Apart from the special questions of cerebral localization,[6] of the trustworthiness of memory, the relation of memory proper to habit or organic memory, the unity of personality, the nature and conditions of "latent

[1] De la suggestion et de ses applications à la thérapeutique, I., Ch. VIII. Paris, 1886.
[2] La mémoire chez les hypnotisées, Rev. Philos., May 1886, p. 441 seq.; also Le sommeil et les rêves.
[3] L'automatisme psychologique, Paris, 1889; also Rev. Philos., XXV., pp. 238–279.
[4] Op. cit.
[5] Étude de la mémoire dans ses raports avec le sommeil hypnotique. Paris, 1887.
[6] For an enumeration of important problems concerning aphasia see The Pathology of Sensory Aphasia, by Dr. Starr. Brain, July, 1889, pp. 95, 96.

memories,"[1] and the processes that occur on the borderland between memory and habit,[2]—apart from these and the more technical questions that cluster around the pathological and biological aspects of the subject, there are yet others. What is the relation of the sensorial after-image, to the memory after-image and of the latter to the ordinary mental image? What are the time-limits and physical conditions of the primary memory? Are the phenomena of recurrent sensations mentioned by Henle and Fechner due to peripheral or central causes? What is the relative accuracy of memory in the field of the different senses? Is there a rhythm in retentiveness as such, or are the variations in the curve of forgetting like those found by Wolfe due to fluctuations of the attention and the like? Do we always remember forwards and not backwards?[3] What are the obscure links in the process of association that the associationist atomism loses sight of? What is the nature of that matrix of presentations which Mr. James Ward calls the "presentation continuum?" and what its relation to the "memory continuum?"[4] Can the memory as such be trained, or do so-called memory-exercises really train only the attention?[5] What degree of memory have animals? What is the genesis of memory in children? and what the relation of great memory to the development of the intellectual powers? These and a host of other problems invite research.

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[1] See Bernheim, op. cit., I., Ch. VIII.

[2] See Beaunis, loc. cit., who found a phase of unconscious muscular memory.

[3] See Bradley: Mind XII., p. 579 seq.

[4] See Encyc. Brit., Art. Psychology, p. 61.

[5] Several recent writers consider the pedagogical aspects of memory. See especially Kay: "Memory, What it is and how to improve it;" C. G. Leland: "Practical Education;" Fauth: Das Gedächtniss; and Eldridge-Green: "Memory, its Logical Relations and Cultivation."

Finally we come again to the deeper philosophic questions. What is the genesis of the power of recognition? and what the explanation of this supreme mystery of memory?[1] Have we not in acts of recognition and comparison forms of consciousness that are not mere functions of its individual elements? What is the relation of attention to the conscious train?

The last question brings us to the front of the battle between the Associationists and the Apperceptionists. The former, under the lead of Bain in England and supported to a considerable extent by empirical studies in France[2] find nothing in the rise and succession of our ideas that may not be explained by the principles of association. The latter with Wundt and Ward at their head lay special stress upon attention and find higher conscious activities that get no adequate explanation from the laws of association.[3] Wundt maintains that apperception, the primal form of voluntary activity, is an essential characteristic of consciousness from the first, that it depends on the whole nature and past of consciousness, and that the will in its function of active apperception directs the conscious train. Recently the contest has extended to the experimental field. Dr. Hugo Münsterberg reports an extended series of experiments upon complicated mental processes the results of which in his opinion clearly refute this apperception theory.[4]

Thus the study of memory—as every psychological study sooner or later must—leads to the subject of attention, and to the deeper mysteries of con-

[1] Space forbids a discussion of theories of recognition and localization. For an excellent discussion of the subject and criticism of recent views, see an article by Fouillée, Rev. des Deux Mondes, July 1, 1885.

[2] See especially Binet's Psych. du raisonnement.

[3] See Ward, loc. cit., p. 41 seq.; and Wundt, op. cit., Bd. II, Ch. XX.

[4] See Op. cit., p. 64 seq.

sciousness; and here for the present this historical paper must end.

APPENDIX.

In closing the historical portion of this study I wish to thank the officials who have aided me at the Harvard and Johns Hopkins Libraries, at the Boston Public Library, the Peabody Library of Baltimore, and the Surgeon General's Library at Washington. I am also under obligation to many others, especially to Pres. G. Stanley Hall, at whose suggestion this work was begun, and to Dr. E. C. Sanford for invaluable suggestions.

I append a bibliography of the most important literature of the subject so far as I have been able to gather it. Only a few titles upon the Association of Ideas are given; and works upon mnemonics, except a few of special interest, have been omitted. Those interested in the subject can refer to the list of books given in Ersch and Gruber's encyclopedia, article *Gedächtnisskunst*, and to G. S. Fellows' bibliography in the American edition of Middleton's "Memory Systems New and Old." Literature upon the pathology of the subject also has been omitted for the most part. For this the articles upon *Aphasia* and *Amnesia* in the catalogue of the Surgeon General's Library should be consulted. Nearly all works upon the general subject of psychology have chapters upon memory. A few of the most important are mentioned in the following list as representative. Nearly all works upon Hypnotism also have some bearing upon memory. For the voluminous literature of this subject see Max Dessoir's *Bibliographie des modernen Hypnotismus*, Berlin, 1888.

Many references of minor importance already given in foot-notes are omitted. In a few cases I have mentioned works that I have not seen when I had reason to believe them of value.

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PSYCHOLOGICAL LITERATURE.

I.—NERVOUS SYSTEM.

Experimentelle und pathologisch-anatomische Untersuchungen über die optischen Centren und Bahnen. (Neue Folge). DR. C. v. MONAKOW. Arch. f. Psychiatrie, Bd. XX, Heft 3.

This paper is based on the detailed study of some five brains, three of them from dogs, from which the visual areas had been removed by Munk. These dogs were observed by Munk subsequent to the operation for considerable periods of time and when killed the brains were turned over to v. Monakow for histological examination —an example of that sort of co-operation in scientific investigation which will assuredly yield good results. The principal discussion relates to the true limits of the cortical visual areas (in dogs), the path connecting this region with the primary optic centres in the corpora geniculata externa, pulvinar and anterior corpora bigemina, and the nature of the connections in these centres on the one hand with the cortex and on the other with the retina. (In considering the latter points the results obtained from dogs, cats and rabbits are combined). In the dogs' brains the cortex for some millimeters cephalad of the visual area, as mapped by Munk, had degenerated, whereas the other portions of the surrounding cortex remained normal up to the edge of the cut. This difference in behavior is regarded as due to the fact that in the operation the coronal fibers for this anterior portion are almost of necessity injured and injury to these would cause the condition observed. In one case also the gyrus fornicatus was found to have a portion of the corpora geniculata externa dependent on it. This observation needs confirmation. At present v. Monakow contents himself with suggesting that the true limits of the visual area should be extended 4 or 5 mm. cephalad, of those given by Munk, especially along the mesial boundary. The fibers connecting the visual cortex with the primary optic centres run in the posterior portion of the internal capsule. When the cortex is removed by the knife there still remains a not inconsiderable portion of the gray matter in the depths of the sulci. As there was every reason to consider this as a part of the visual cortex and as at the same time it did not appear to be functional in operated dogs, Munk has assumed that it has degenerated as a consequence of the injury. v. Monakow however finds it in fairly good condition in several cases and hence some other explanation is needed for its inactivity.

Following the removal of the visual area changes take place in all three primary centres. In dogs and cats, the degeneration is found in the entire ventral and the cephalic portion of the dorsal nucleus of the corpora geniculata externa; in the caudal and ventral portions of the pulvinar and in the superficial gray and middle fiber layer of the anterior corpora bigemina. (By way of explanation it should be understood that v. Monakow distinguishes five cell groups

in the corpora geniculata externa. They form a dorsal group of two lying above a ventral group of two, which in turn is above the single ventral nucleus. The retina is specially connected with the caudal-dorsal nucleus while the other four are connected with the cortex. They are the frontal-dorsal caudal-ventral, frontal-ventral, and the ventral nuclei). In newborn dogs and cats the atrophy passes beyond the primary centres and involves the optic tracts, the optic nerves and the cells of the granule layers of the retina. In rabbits there is atrophy of the entire dorsal with slight atrophy of the ventral nucleus of the corpora geniculata externa, while the external fibre layer remains intact; atrophy of the ganglion cells and ground substance of the pulvinar and, in the anterior corpora bigemina of the middle fiber layer.

Section of the visual fibers in the internal capsule gives peripherally the same result as the removal of the visual area. At the same time there is an ascending degeneration which involves the large cells of the third layer of the cortex and the nerve networks in the region of the third and fifth layers.

Enucleation of the bulbi oculorum, in newborn animals is followed, in dogs, by atrophy in the gelatinous substance of the lateral zone of the corpora geniculata externa, together with that of the ganglion cells, especially in the dorsal-caudal nucleus of this body, also in the dorsal-caudal portion of the pulvinar. In the dog the disturbances in the anterior corpora bigemina are doubtful. The changes in the cat are similar to those in the dog save that the disturbance in the anterior corpora bigemina are more evident. In the rabbit finally the gelatinous substance of the lateral zone of the corpora geniculata externa atrophies, the cells of this body remaining fairly normal. The pulvinar shows no changes (?) but in the anterior corpora bigemina there is atrophy of the superficial layer of fibers with disappearance of the cells from the superficial gray layer. After removal of one of the anterior corpora bigemina, in the cat or rabbit, the tractus opticus becomes smaller and in the optic nerve the finer fibers degenerate, while the coarser ones remain intact (v. Gudden and v. Monakow). In newborn cats, in which an entire hemisphere has been removed and also the tractus opticus of the same side cut, a group of cells in the caudal-dorsal region of the corpora geniculata externa remains, while all other groups, together with those of the pulvinar, disappear (v. Monakow). After section of one optic tract in a rabbit there is atrophy in both nervi optici (insignificant, of course, in that of the same side), and in the corresponding portion of the retinae disappearance of the large ganglion cells of the deep layer. (Ganser).

These facts v. Monakow attempts to knit together so as to present in a scheme something of the relation of cells and fibers along the entire line of the optic pathway. In doing this he considers it as proved that, in newborn animals at least, section of systems of fibers causes them to degenerate in both directions, that after such a section certain groups of ganglion cells are fully destroyed while others are only slightly affected. He also accepts the two categories of cells according to Golgi, but agrees with Forel in considering those, the axis cylinder of which soon loses its identity and forms a network, as central or intermediate cells, serving to put into connection those of the first category. Physiological connection between the branches of a net is considered as due rather to the contiguity than the continuity of the finest branches. The cell groups that do

not degenerate on the section of the fibers are identified as intermediate cells (second category). Not to go into the details of the scheme we give his general statement of the result. "In each optic centre one system of fibers arises and one ends and the several systems of projection fibers are united with one another by intermediate cells. Both in the primary and in the secondary portions of the optic pathway two fiber systems run parallel and in opposite directions and groups of principal cells regularly alternate with intermediate ones." Or putting the facts in another way: in the centripetal direction we have (in the rabbit), the large ganglion cells of the retina, their axis cylinder processes passing in the optic nerve and terminating in a net-work in the lateral zone of the corpora geniculata externa. Here they make indirect connection with the intermediate cells which in turn are connected with the principal cells of this body and of the pulvinar. From these latter points fibers run to the fifth (and third) layer of the cortex in the visual area, there forming a network that connects with the cells forming the above mentioned layers and these in turn connecting with other elements (solitary cells) of the cortex. In the centrifugal direction we have the large pyramid cells of the third cortical layer, their axis cylinder prolongations passing ventrad as the fibers of the visual area and forming a network in the corpora geniculata externa (where?) and in the anterior corpora bigemina and through the intermediate cells in the latter, making connection with the principal cells in the superficial gray layer. The axis cylinder prolongations of these last give origin to the fine optic fibers which form a network in the retina and connect there with the granule layers.

This scheme is put forward by the author as tentative, but he expresses the hope that in the main it may be found correct. The idea of double tracts throughout the optic pathway is certainly striking, but in view of the problematical nature of the retina and optic nerve from a morphological standpoint, certainly worth further study. It must be remembered that as presented the scheme applies strictly to rabbits only.

On his way to these conclusions v. Monakow makes a number of interesting observations some of which may be briefly referred to. Goltz has pointed out that the dog without eyes differs much from the dog without visual centres, the latter exhibiting a marked disinclination or inability to go up and down steps or jump from a height, acts which the dog merely without eyes does with ease. A dog without eyes, which v. Monakow observed, and about whose general condition of health there could be no doubt, learned to go up and down stairs only with much difficulty and could not be persuaded to jump from a low bench. This same dog followed the movements of the children who kept him with such ease, was so active in play and in so many ways masked his defect of vision that v. Monakow refers to the case of the rabbit which had lost its visual centres on both sides and yet acted quite as if it could see (v. Gudden), remarking that the tests were such that, if applied to the blind dog just mentioned, they would prove that he too could see. This criticism is however not conclusive. There seems little doubt that certain birds can see without the aid of the cerebral hemispheres and among the cold blooded vertebrates the primary optic centres appear quite independent. It is still a fair field for investigation to determine whether among the mammalia such independence can exist.

On the anatomical side v. Monakow notes that in the vertebrate

series as we descend, the anterior corpora bigemina or their homologues, the optic lobes, become more and more important as optic centres until they are finally the exclusive centres for vision. As regards the history of the degenerative process v. Monakow finds that in the pulvinar and the anterior corpora bigemina the degenerative process after removal of the visual area first attacks the ganglion cells and only secondarily and later is the ground substance involved. Further, that the stellate cells are comparatively early productions in the process, being absorbed in the later stages.

A continuation of the paper is promised.

Makroscopische pathologisch-anatomische Hirnpräparate. DR. C. v. MONAKOW. XIV Wanderversammlung südwestdeutscher Neurologen und Irrenärzte, Mai, 1889. Abstract in Neurolog. Centralbl, No. 13, 1889, by Dr. L. Laquer.

v. Monakow exhibited the following preparations:

1. Brain of dog from which on the day of birth there were removed the entire occipital and a portion of the temporal lobes on the left side. Death after eight weeks, corpora geniculata externa, pulvinar and corpora geniculata interna on the left side much atrophied. Left optic tract up to chiasma considerably reduced in volume. Both nervi optici slender.

2. Brain of a dog from which the larger part of the right cerebral hemisphere was removed three days after birth. Besides the frontal end and the olfactory lobe only fragments of the gyrus fornicatus and sigmoideus and of the temporal lobes remained behind. The internal capsule was completely severed. Death after 8½ months. There was found great shrinkage of the thalamus opticus, corpora geniculata externa and corpora geniculata interna on the right side; evident flattening of the right anterior colliculus of the corpora quadrigemina and the right corpus mammillare. Right pyramid had completely disappeared and the pons on the right side was flattened. Right optic tract as far as the chiasma reduced to half the size of the left one. Both nervi optici small but not macroscopically very different from one another. The region of the nuclei of the columns of Goll and Burdach slightly depressed on the left side; corpus callosum very thin. The cerebellar hemispheres quite normal and similar but the hemisphere on the right side had expanded somewhat into the cavity left by the removal of the occipital lobe on that side.

3. Human brain extensive region of softening in the left occipital lobe. Patient a painter, 68 years of age, who suffered from hemianopsia and alexia. The medullary substance in the region of the angularis, the first occipital convolution and the caudal-dorsal portion of the præcuneus for the most part destroyed and absorbed, giving rise to an extensive cyste. The softening nowhere reached the sagittal fibers. Posterior cornu much distended, cuneus and the second and the third occipital convolutions intact. The same was true for the ventral portion of the bundle of Gratiolet. The cortex of the gyrus angularis appeared quite normal. Secondary degenerations were observable in the dorsal portion of the bundle of Gratiolet on the left side as far as the lateral fiber layer of the pulvinar. Considerable secondary reduction in the pulvinar and corpora geniculata externa on the left side. Atrophy of the arm of the anterior colliculus and of the caudal portion of the left tractus opticus.

The right optic nerve as large as the left but somewhat gray on its mesial side.

4. Brain of an idiot about 28 years of age who at the age of two had experienced an embolism of the left arteria fossæ Sylvii. On the left side the first temporal convolution is wanting and the ventral portion of the left parietal lobe, the medullary substance having especially suffered. Lateral ventricle much distended. Secondary atrophies; left pyramid very slender. The median nucleus of the thalamus opticus and the tuberculum anterius much diminished, while the pulvinar and the corpora geniculata externa corresponding with the fairly normal condition of the occipital lobe are almost as well developed as on the right side. The left corpus geniculatum externum has however almost completely disappeared. The corpora striata are normal on both sides. v. Monakow then expressly called attention to the fact that the intimate connections shown by him to exist between the various portions of the cortex and the inter and mid-brain were true not only for the rabbit, but also for the dog and man, being indicated by the secondary degenerations following cortical lesions. The dependence of the corpora geniculata externa on the cortex of the temporal lobe is for the first time described for man, but corresponds with observations previously made on both the rabbit and cat. The cases 1 and 3 also illustrate the necessary degenerations of the primary visual centres after the injury of the occipital lobes.

Ueber Befunde bie Erkrankung des Hinterhauptslappens. HOELE. Berliner Gesellschaft für Psychiatrie und Nervenkrankheiten. Sitzung vom Juli 8, 1889. Rev. in Neurolog. Centralbl. No. 14, 1889, by Hadlich.

The author presents observations of three cases in which there was disturbance of vision during life and the autopsy showed lesions in the occipital lobes, with which these symptoms were associated. At the same time the optic pathway from the cortex to the eye was more or less involved showing signs of atrophy. The results were presented as a contribution to the localization of vision in the occipital lobes.

Ueber die centralen Organen für das Sehen und das Hören bei den Wirbelthieren. HERMANN MUNK. Sitzungsber. d. Königl. Preuss. Akad, d. Wiss. zu Berlin. XXXI. 20 Juni. 1889.

This condensed account of Munk's views on some disputed points forms the fifth and closing paper of a series which commenced in 1883. In it he presents, besides his critical remarks, the main results of further work on dogs, monkeys and newborn rabbits.

In operating dogs and monkeys he recommended that the portion of the brain removed be cut out in one piece, one advantage of this method being that it enables the experimenter to see pretty accurately what the extent of the initial lesion is. The results are similar to those obtained by his previous procedure. These in the main have been corroborated by Sanger Brown and E. A. Schäfer and by Vitzou from the experimental side, while from the pathological side Nothnagel furnishes evidence for a similar relation of the cortex in man. Munk extends his results to the lower mammals, on the ground that though the visual centres are not well made out there, yet in rabbits,

guinea-pigs and rats, the removal of an entire hemisphere causes contralateral blindness. In discussing the results of Schrader on pigeons (see AMER. JOUR. PSYCHOL., vol. 2, p. 471) which are opposing—for Schrader claims that pigeons without the forebrain can still see—Munk urges as a chief objection that the cortical substance was not entirely removed, and if some remained it would account for what Schrader observed. Deciding against him, he concludes that the relation of the forebrain to vision is fundamentally different in the mammals and birds (?) to what it is in frogs and fish, and that the former without their forebrain are entirely blind. Further, that in the former the very sensation of light has its seat in the cortex, and that the optic or retinal reflex—the narrowing of the pupil when light is directed on the retina—does not require even the sensation of light for its performance, and cannot therefore be adduced as an argument.

The final portion of the paper is a criticism of Wundt's position on this question. Wundt argues that the (central) cortical elements are functionally indifferent, and the characteristic sensations which they give are due to molecular disturbances of a special sort dependent on the nature of the stimulus which has excited the nerve. In other words, if it were possible to connect one sensory nerve with the cortical centre of another sensory nerve, the sensation following a stimulation of the nerve would depend on the nerve stimulated, not on the centre with which it was connected. Such a view is based mainly on the apparent restitution of function after operations on the cortex, on the possibility of removing large parts of the hemisphere with only slight disturbance of function as a result. The other objection is the theoretical one that either the specific energy of the cortical cells was present from the earliest stages of development, which is highly improbable, or it has been gradually developed, in which case it is not strictly specific. In criticising the above, Munk points out that there is no good evidence for the notion that specific energy of the cortical cells depends on peculiar vibrations of the nerves to which they are attached, and that experiments on new-born animals, which should support this idea if any experiments would, are in fact opposed to it. As to the second objection, it might be urged against the physiological division of labor throughout the entire organism, and is not a special difficulty of the doctrine of the specific energy of nerves.

(The somewhat rigid form which Munk gives to his ideas on localization has always excited opposition because it is opposed to the gradation of function which is characteristic for living things. As regards the visual centres, he will have all mammals like the dogs and monkeys which he has operated; and yet for rabbits, guinea-pigs and rats he admits that the visual centres have not been defined, and for the last two bases his conclusions on those which have lost an *entire* hemisphere. In discussing v. Gudden's rabbit which had lost the posterior portions of both hemispheres, he states that undoubtedly that rabbit could see, but adds that other rabbits which in addition have had the cortex for some two mm. further cephalad removed were blind, and blames v. Gudden for his assumption that he had by his operation completely removed the visual centres. In the case of Schrader's pigeons, the objection is urged as one of great importance, that small portions of the cortex had been overlooked and were not removed. The above is prefatory to the question: what is the significance of the small portions of

cortex? The animals in question are not reported as seeing slightly with a small portion of the retina, but guide and control their motions with some skill. Either, then, their sight is better than the portion of cortex remaining would indicate, if the relations are the same as in dogs and monkeys, or a small portion of cortex comes to possess unusual powers as a visual center. Indeed, the attitude of Munk and Flourens with regard to the controlling value of a small part of the cortex is, when viewed in the light of the above experiments alone, by no means so different. Certain it is that the dependence of the centres in the spinal cord on the motor portions of the cortex decreases as we pass from man down the vertebrate series, and this fact suggests the possibility of a similar relation between the primary sensory centres and the cortical areas belonging to them. Such an idea would imply in the lower mammalia some power of the primary centres to act even after the cortex was destroyed. Against such a view Munk protests vigorously. But leaving this point one side for the time, it is most important that some demonstration of the value of residual portions of the cortex should be made, to determine whether the degeneration in the primary centres is quantitatively proportionate to the amount of cortex removed, or whether a small portion of cortex exercises a nutritional influence over these centres which is disproportionate to its size, or, whatever the relation may be, to give it some anatomical basis. REV.).

Ueber den Bau des Säugethiergehirns. Vorläufige Mittheilung von DR. G. JELGERSMA in Meerenberg, Niederlande. Mit 1 Tafel, Morph. Jahrb., 1889, 15. Rev. in Neurolog. Centralbl., No 14., 1889, by P. Kronthal.

The author describes the intellectual tracts and centres of the medulla oblongata and cerebral axis. The course of the tracts is as follows. Starting from the cerebral cortex and passing through the internal capsule and the two lateral portions of the pes pedunculi, they unite in part with the nuclei of the pons on the same side and in part pass on to the ganglion cells of the nucleus olivaris. Both sets of fibers then cross with the corresponding ones on the other side. The crossed fibers from the nuclei of the pons pass through the brachium pontis, those from the nucleus olivaris through the corpus restiforme, the former going to the cortex of the cerebral hemispheres, and the latter probably to that of the vermis cerebelli. The cerebellar cortex is further connected with the nucleus dentatus. From this arises the superior peduncle of the cerebellum, which in turn, at the level of the corpora quadrigemina crosses with that of the other side, to unite with the nucleus ruber. From this nucleus fibers pass to the optic thalamus and the capsula interna, to finally end in the cortex.

The differences between man and the monkeys with regard to this entire tract are considerable: in man, the great development of the cerebellar hemispheres; in the monkey, the exposure of the corpus trapezoides on the cephalic side, absence of the neucleus arciformis, meagre development of the nucleus ruber, of the superior peduncle of the cerebellum, of the nucleus olivaris and the nucleus dentatus. The difference in the development of the intellectual tract seems to be as great as that of the surface of the brain. In comparing brains of different classes of animals, however, with reference to this tract,

it became plain that the high or low organization of the brain could thus be only very roughly determined

The author correlates the development of the vermis with that of the axial ganglia, and concludes that the function of the cerebellum is psychic. Regarding the formation of the convolutions Jelgersma concludes that it is independent of the forces outside of the brain itself, and that both in the cerebrum and cerebellum the formation of convolutions is due to a localized tendency to superficial growth and mutual accommodation between the gray substance and the conducting white matter.

Monstres Cyclopes. C. PHISALIX. Journal de l'Anatomie et de la Physiologie, etc. Janvier, Février, 1889.

The monsters are all mammalian and the cyclopean type is illustrated by one case from man, one from a dog, and two from sheep. The parts concerned are carefully and minutely described with a view to determining to what extent the abnormalities are correlated. The conclusion of the argument is that nutritive, not mechanical causes must be called in to explain these cases; and it is urged that with this point in view monsters should be studied histologically. From the anatomical side it is made out that the cyclopean condition is always associated with arrested development of the fore-brain; and that the relations of the choroid plexus show that the plexuses of the lateral and third ventricles, which in the adult are in connection, have an independent development and become fused later. In the case of the dog sensory branches were found arising from the fourth nerve (patheticus), thus supporting Phisalix view on the spinal type of some of the cranial nerves. (See AM. JOUR. PSY., Vol. 1, p. 492.)

Experimenteller Beitrag zur Kenntniss der Hörnervenursprungs beim Kaninchen. Prof. BUMM. Jahressitzung des Vereins der deutschen Irrenärzte 1888. Allg. Zeitschr. f. Psychiatrie, etc. Bd. 45, Heft 5 and 6, 1889.

Four rabbits were operated three days after birth. In *A* and *B* the left acusticus was cut; in *C* the left cerebellar hemisphere removed, and in *D* the vermis. *A* and *B* were killed after three weeks, *C* after six months, and *D* after six weeks. Examination of the posterior auditory root in *A* showed, according to the author, that this root rises from the tuberculum acusticum and the anterior auditory nucleus (terminology of Forel-Onufrowicz) and that both these ganglia are also in connection with the fibres of the corpus trapezoides. Rabbit *B* showed that the anterior auditory root rose (in part) from the cells and network ventrad of the nucleus of Deiters. It is concluded from *C* and *D* that the posterior auditory root is not connected with the cerebellum, whereas the anterior has a partial origin somewhere in the vermis. On the central paths of the auditory fibres his specimens throw no light.

The relations between the superficial origins of the spinal nerves from the spinal cord and the spinous processes of the vertebræ. R. W. REID. Journal of Anatomy and Physiology, Vol. XXIII, April, 1889.

Taking the spinous processes as his landmarks, Reid, by careful dissection of six subjects, has gotten a series of results which are

presented in two tables and two plates. He first determined that the variation in the level of origin between the sensory and motor nerves of the same segment did not exceed three mm. in any case. In making the investigation then the cord was exposed so as to show the posterior nerve roots and at the same time leave half the spinous process in place. Five of the specimens were adult males, one an adult female. The first table gives the upper and lower limit for each posterior root. The second table is condensed from the first to give the maximum variation, and the second plate graphically represents the results in the second table. The result shows great variability in the superficial origin of the spinal nerves when referred to the spinous processes.

On nerve tracts degenerating secondarily to lesions of the cortex cerebri. C. S. SHERRINGTON, M.A. (Preliminary communication). Journ. of Physiology, Vol. X, No. 5., July, 1889.

In 1884 Sherrington worked over some material put at his disposal by Prof. Goltz with a view to finding whether in dogs the location of the lesion in the cord could be connected with the portion of the "cord area" (*i. e.*, that region of the cerebral cortex the removal of any part of which causes a degeneration in the spinal cord) which was removed. He soon found that the appearance of the cord lesion was so dependent on the time elapsing between operation and autopsy that it was first necessary to study the variations due to this factor. In the present paper he returns to the original question of determining "to what extent there is in the pyramidal tracts a grouping of nerve fibers corresponding to the grouping of nerve cells in the cord area of the cerebral cortex." The experiments were mainly on monkeys (Macacus). In these animals the pyramidal tract was found to occupy three fourths of the transverse area of the crusta a little above the pons; and to extend as low as the origin of the cocygeal nerve roots. It was further noticed in the cord of Macacus that from the level of the 2d lumbar nerve roots a considerable portion of the tract lies outside the direct cerebellar tract, thus separating the latter from the periphery of the lateral column. "After a cortical lesion of less than 30 square mm. extent the degeneration in the cord was found scattered over the whole transverse area of the tract." This furnishes a negative answer, therefore, to the initial question. The only grouping among these fibers passing from cortex to cord, was indicated by the fact that the more mesial and anterior cerebral lesions were followed by degeneration which (in the cervical cord) abutted on the direct cerebellar tract, whereas following removal of the more posterior and lateral cortical portions the degeneration about the cerebellar tract was less evident.

Further examination indicated that the "re-crossed" pyramidal fibers were well developed in the monkey in the cervical and lumber enlargements. The "re-crossed" tracts are continuous with the fibers that have once crossed in the pyramidal decussation and are formed by fibers which pass back again to the side from which they came. According to Sherrington, they are probably due to branching of the crossed fibers. Fibers thus branching are designated as "geminal fibers," and their existence is supported by the fact that fibers are frequently found in pairs in the same stage of degeneration; that two axis cylinders are found in one sheath, and that the geminal fibers are most abundant in the crossed tract.

There are other results given, one of which is that "interhemispheral degenerations tend to scatter and do not pass between identical areas of the two hemispheres."

Vergleichend-entwickelungsgeschichtliche und anatomische Studien im Bereiche des Centralnervensystems. II. Ueber die Fortsetzung der hinteren Rückenmarkswurzeln zum Gehirn. Dr. L. Edinger. Anatomischer Anzeiger, No. 4, 1889. Rev. in Neurolog. Centralbl, by P. Kronthal.

As the result of his own investigations on the spinal cord of the frog, Edinger has determined that voluminous fiber bundles arise from the posterior cornua and pass ventrad, crossing with those of the opposite side, both ventrad and dorsad of the central canal. After decussation these fibers pass cephalad for the most part in the anterior and to a small extent in the lateral columns. This crossing occurs in mammals also, but in man has escaped observation because it is inconspicuous in frontal sections. The path of the sensory nerves is, therefore: peripheral end organ, nerve, cell of the spinal ganglion, posterior root, union with a second nucleus, decussation, continuation to the lemniscus, whither the antero-lateral columns pass. The view that all the sensory fibers do not run in the posterior columns is supported by observations in tabes as well as by physiological experiments.

Progressive neurotische Muskelatrophie. Dr. J. Hoffmann. XIV Wanderversammlung südwestdeutscher Neurologen und Irrenärzte, Mai, 1889. Abstract in Neurolog. Centralbl, No. 13, 1889, by Dr. L. Laquer.

To the disease described by him under this name Hoffmann adds some anatomical facts. He cites several cases where the anatomical changes have been recorded and concludes that the following are made out: centripetal degeneration of the motor and sensory peripheral nerves, similar degeneration of the anterior and posterior spinal nerve roots, degeneration of the posterior columns in the lumbar region—from there cephalad only the columns of Goll are involved—, atrophy and disappearance of the multipolar ganglion cells of the anterior cornua, with changes in the muscles as elsewhere described. Neither amyotrophic lateral sclerosis nor the various forms of poliomyelitis anterior nor Friedrich's disease (hereditary ataxia) nor ependymal sclerosis nor multiple neuritis produce such changes. From the lesion of tabes dorsalis it is distinguished by the immunity of the columns of Burdach above the lumbar region. (In a case quoted from Gombault and Mallet it is stated that the degeneration of all the fibers connected with the posterior root ganglion takes place while the ganglion cells remain normal! Rev.)

Anatomische Untersuchung eines Falles von amyotrophischer Lateralsklerosis. Dr. Otto Dornblüth. Originalmittheilung, Neurolog. Centralbl. No. 13, 1889.

This interesting case is given with some detail. At the end of the article there is a summary from which we take the following:

In a woman of 58 years with hereditary taint, and who had suffered from folie circulaire for 4 years, and exhibited increased muscular irritability for some time, there suddenly appeared paralysis of the

upper extremities which soon decreased. After two years there were contractures in both legs. There were peculiar rises in the temperature, lasting a short time, immobile pupils and general atrophy. After two years more contractures in both arms; decubitus, death. The autopsy showed the lesion for amyotrophic lateral sclerosis. The degeneration in the pyramidal tract could be followed from the lumbar region to the level of the corpora quadrigemina at which the process was the more recent. The motor cells of the cord were degenerated, also the nuclei of the hypoglossus, anterior vagus and glossopharyngeus, facialis, and motor trigeminus. Moreover there was hyperæmia of the gray matter along the floor of the medulla and of the pons, fresh extraversations specially in the region of the posterior vagus nucleus (sensory) an old extraversation cephalad of the facialis knee, with secondary ascending atrophy of the right posterior longitudinal bundle. This case agrees with those previously reported in showing motor ganglia and tracts to be alone involved by the disease. The degeneration of the pyramids disappeared caudad of the internal capsule. (The brain was not examined microscopically, but showed no lesion at the autopsy). It is suggested that the case may illustrate Westphal's generalization that fiber systems which acquire their medullary sheaths the latest are those which most easily undergo involution, or that some toxic substance exercises a selective action on motor cells and fibers. The variations in temperature may have been connected with the extraversations into the sensory nucleus of the vagus.

Recherches sur la structure des cordons postérieurs de la moelle épinière de l'homme. N. POPOFF de Varsovie. Archives de Neurologie. Mars, 1889. Rev. in Neurolog. Centralbl., No. 16, 1889, by Nonne.

Testing Becterew's statements on the components of the column of Goll, Popoff finds by way of confirmation that the group of fibers nearest the middle line becomes medullated earlier than the more lateral portion. The difference is clearer in the cervical than in the dorsal region. According to Popoff the mesial portion originates from the column of Clarke, the lateral from the posterior commissure.

A gerinczvelöi idegek hátulsó gyökereiröl. M. LENHOSSÉK. Ung. Akademie, Sitzung v. 20 Mai, Auszug aus Orvosi Hetilap 1889, p. 21. Rev. in Neurolog. Centralbl, No. 16, 1889 by Jendrássik.

The course of the posterior spinal roots was studied by means of comparative anatomy and embryology. The entering fibers divide into three groups. A mesial (*innere*), intermediate (*mediale*), and lateral portion. The two former acquire their medullary sheaths earlier and are composed of larger fibers than the last. The mesial group passes through the substantia gelatinosa in a number of thick bundles, and bends to form the longitudinal bundles of the posterior cornua (Kölliker.) From here, after a longer or shorter course, they pass ventrad for the most part in the direction of the lateral cells of the anterior cornua, smaller portions to the lateral column and posterior commissure. This last group is small in man but more developed in the cord of the guinea-pig.

Of the intermediate group, a portion enters the column of Bur-

dach and, after a longer or shorter course there, passes ventrad to the gray substance. Another portion passes directly through the substantia gelantinosa, ending in a network in the lateral cells of the anterior cornua; another portion to the column of Clarke and the posterior commissure.

The lateral group first passes longitudinally as the marginal zone of Lissauer, then ventrad through the zona spongiosa. The columns of Burdach consist of fibers from the intermediate group. The longer fibers pass from the lumbar roots to the cells of Clarke in the dorsal cord and a portion of the fibers pass from the cervical roots caudad to the same cell group, the major portion however passes to the nucleus of this column in the medulla. The Posterior Commissure contains: 1. fibers from the mesial and intermediate groups, medullated in embryos only 36 cm. long; 2. fibers arising from cells in the galatinous substance of Rolando becoming medullated after birth. One difference between these results and those of other authors is that Lenhossék traces none of the fibers from the posterior roots into the anterior commissure.

Zur Frage über Veränderungen der Nervencentren bei peripherischen Reizen. S. Ssadowski. Dissertation, St. Petersburg, 1889. Russisch. Rev. in Neurolog. Centralbl, No. 15, 1889, by P. Rosenbach.

The question to be solved was whether pathological changes could be induced in a ganglion by the stimulation of its peripheral nerve. The experiments were made on dogs and rabbits. Ssadowski stimulated on one side of the body, the nervus ischiadicus, vagus, auricularis magnus, or intercostalis. The stimulus was either by faradization or ligature of the nerves. In the first case the stimulus was applied for fifteen min. daily through several weeks. In the last the ligature was applied for a period of from 7—70 days. The animals were then killed and a microscopic examination made of the nerve and the associated ganglion. The nerve trunk in the neighborhood of the ligature showed evidence of degeneration; that which had been electrically stimulated did not. On the other hand, the ganglion in both cases showed atrophic degenerative changes (consisting in vacuolization, coagulation necrosis, and shrinkage) of the nerve cells, and at times infiltration with lymphoid elements and distention of the capillaries. These results are explained by the author as degeneration following disturbance of nutrition which was in time caused by excessive stimulation.

Further observations on the histology and function of the mammalian sympathetic ganglia. W. Hale White, M. D. Journ. of Physiology, Vol. X, No. 5, July, 1889.

The study of the superior cervical ganglia in the human adult and comparison of it with other forms led White to conclude some time since that in man at maturity these ganglia were functionless. (See abstract Am. Jour. Psy. Vol. 1. p. 329.) In the present paper he concludes from a study of the same ganglia taken according to the age of the subject that there is a progressive degeneration of the cells from birth on. He has further examined the semilunar ganglia in a similar way and finds the same general relations all around that were determined for the cervical. In examining the thoracic ganglia they are found much more constant in size and in general more

normal, or at least never so degenerate as the first two that were studied. He concludes as the result of his investigation up to this point: firstly, that in lower mammals and young human beings the collateral ganglia (if we may judge from the superior cervical and semilunar) are functionally active, but that in monkeys there are evidences of commencing loss of function, which has completely disappeared in the human adult; secondly, that in man the function of the lateral ganglia is maintained well into adult life and only begins to disappear in old age. It is a curious fact that in all these cases the sympathetic nerves are described as normal. The possibility of degenerate ganglia associated with normal nerves in the sympathetic system is not explained by any existing view of the relation between cells and fibers in that region and at first sight, at least, is one of the most striking results.

On the Minute Anatomy of the Vagus nerve in Selachians, with Remarks on the Segmental Value of the Cranial nerves. THOMAS W. SHORE. Journal of Anatomy and Physiology, Vol. XXIII, pp. 428—451. Plates XX—XXI.

In a former paper (noticed in this JOURNAL Vol. II, p. 309) the author gave a summary of our present knowledge of the anatomy and development of the vagus in Petromyzon, Elasmobranchs, Rana and Amniota. The present paper contains the results of the author's researches upon the microscopic anatomy of the vagus of the skate (*Raia batis* and *R. clav.*). The nerve cells of the vagus of the skate are arranged in five groups. The nerve does not contain any non-ganglionated somatic motor fibers, and there is only one small faciculus of ganglionated somatic sensory fibers, viz., the small dorsal branch. The splanchnic motor and probably splanchnic sensory fibers are well marked, and are, as in the case of a typical spinal nerve, divisible into a non-ganglionated portion, which runs chiefly in the post-branchial branches, and a small-fibred ganglionated part, which is found in the branchials and visceralis. The vagus nerve of the skate, therefore, does not contain all the elements of a single perfect spinal nerve-metamer. It contains the typical elements of the so called sympathetic system, namely, splanchnic small medullated fibers some of which join a proximal set of ganglia, others passing on to a distal set. The proximal set of ganglia are represented by the branchial and viceralis ganglia, the distal set by the pre-branchial ganglia of the skate's vagus. F. T.

A demonstration of centres of ideation in the brain from observation and experiment. BERNARD HOLLANDER. Reprinted from the Journal of the Anthropological Institute, (London,) August, 1889.

The author attempts to correlate the modern experiments of the brain physiologists with the older observations of the phrenologists. Some half dozen "organs" are thus identified with the "centres" on the general principle that the "organ" is located in the region, where stimulation of the cortex gives rise to movements, gestures or facial motions that are expressive of the feeling for which the organ stands. The method pursued in correlating the two is however unscientific. Judging by the "discussion" at the end of the paper it was nevertheless received without any severe criticism. A paper of the same import was read by the author before the Anthro-

pological Section of the British Association for the Advancement of Science.

Heat Centers in Man. ISAAC OTT, M. D. Brain, January, 1889.

From the study of the lower animals the author recognizes six centers, injury to which causes increased temperature. These are described as the cruciate, about the Rolandic fissure, the Sylvian at the junction of the supra and post-Sylvian fissures, the caudate nucleus, the tissues about the corpus striatum and the optic thalamus near the median line, and the anterior end of the optic thalamus itself. A few cases are presented which are considered to indicate the existence of similar heat centers in man, and in the closing sentence the task of localization with the required precision is said to devolve upon neuropathologists.

II.—EXPERIMENTAL.

COLORED SHADOWS.[1]

Whenever an object is illuminated from two different directions by lights which are approximately equal in intensity but more or less different in color, the shadows thrown by the object, especially if they fall on a white ground, will be colored. The difference in shade may be so slight that we ordinarily regard both lights as white, as in case of candle and moonlight, yet the colored shadows will appear. When the light from one source is white, its shadow is of the color of the other light; for it is illuminated by the latter light alone. But the other shadow, although it is illuminated only by white light, yet always appears of a color complementary to that of the neighboring field, which is illuminated by both lights together.

These shadows may be obtained in various ways, and they are of by no means infrequent occurrence. We see them in the theatre when colored lights are used, or in our room if we have a colored shade for our lamp. We notice them sometimes on the page we are reading, when the window shade is partly drawn, so that the daylight enters partly uncolored through the window, and partly slightly tinted through the shade. Goethe and others speak of beautiful effects caused by the setting sun shining on snow-covered fields. For experimental purposes the best plan is to use a dark room arranged in a manner soon to be described.

The color of the complementary shadow is at first sight hard to account for, and has given rise to much discussion. Helmholtz and others think that it is an effect of contrast with the neighboring field, due to a "deception of judgment," and that colored shadows form an *experimentum crucis*[2] of the psychological nature of color-contrast. There are certain facts connected with the shadows, they maintain, which entirely overthrow the physiological theories. We hope to show that in this they are wrong; that all the phenomena presented by these shadows can be explained at least equally as well —many of them, it seems, very much better—by a purely physiological theory.

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[1] Written for the Graduate Course in Psychology at Harvard University.

[2] Von Kries, "Gesichtsempfindungen," p. 131f

In the first place, the color of the shadow is evidently in some way due to the presence of the colored light which surrounds it; for it is always complementary to the latter, and if no colored light is present, the shadow appears grey. Yet in every case it is illuminated by white light only, and hence objectively does not change in color.

Its color then must be due either to retinal fatigue or to contrast of color. Helmholtz claims that it is primarily and especially the latter; for the color is seen at once if one takes care to regard the shadow without having previously looked at the surrounding field, or allowed the colored light to affect the retina. Its color is intensified, however, by aid of fatigue, if the eye is allowed to wander about between the shadow and the colored field. The facts which he and others rely upon to prove conclusively that this contrast is due to psychological, and not to physical or physiological causes, are described by him as follows. His shadows are produced by using candle-light, which is reddish yellow in color, and weakened day-light. They are therefore colored blue.

"Take a tube, blackened inside, and place it so that when one looks through it the eye sees only parts of the paper which lie in the shadow of the candle-light. Let at first only day-light fall upon the field, then look through the tube, and then allow the candle-light to enter. One sees now nothing of the parts illuminated by the candle-light, does not notice its presence, and the appearance of the portion of the paper which he sees through the tube remains unchanged. It follows that objectively the color of the paper in the shadow of the candle-light is unchanged.

"If however the observer directs the black tube through which he is looking so that a portion of the field which is being observed is lighted by the reddish-yellow light of the candle, then the shadow of the candle-light becomes blue. When the blue has very intensely developed itself, turn the tube again so that nothing but the subjective blue is in the field of view. Now the blue remains whether we allow the candle-light access to the rest of the paper or cut it off entirely,—which of the two we do is indifferent to the observer, since under these circumstances he sees nothing of the part of the field which is illuminated by the candle, and he cannot know whether its light is present or not. The blue color in such a case is so persistent that Osann decided it was objective. This assumption is easily disproved, in that the blue color still remains when the candle-light is extinguished. In the moment however when one removes the black tube from the eye, the subjective blue disappears, since one recognizes it as identical with the white which fills the rest of the field of view. No observation shows more strikingly and plainly the influence of the judgment on our color sensations. After the judgment is once fixed in consequence of the contrast, whether it be successive or simultaneous, that the color in the shadow of the candle-light is blue, the color still remains apparently blue even when the circumstances which have led to this decision are removed; until by removing the tube we make possible a new comparison with other colors, and through new facts allow our judgment to be otherwise determined."[1]

The nature of this "deception of judgment," which leads us wrongly to think the shadow colored, is concisely described by

[1] Helmholtz, "Physiologische Optik," p. 394f.

Delbœuf in speaking of these experiments. The experiment proves, he thinks, "that the sensation of color can depend upon an act of thought alone."[1] If we look through a pane of red glass, rays which come through it to the eye from a white object will be red. Rays coming from a green object, if the conditions were perfect, would be entirely cut off, and the object would appear black; but the red glass always lets through more or less white light, and with every green is mixed some white; so that some grey rays would come to the eye from a green object. Nevertheless we do not judge the white object to be red, nor the green to be grey, but they appear to us more or less in their natural colors. This is because we are accustomed to judge of the colors of objects through varying modifications of the light which surrounds us, and which changes continually according to the state of the atmosphere, the aspect and disposition of the clouds, the color of reflecting objects, etc. Hence we have learned to judge correctly the color of objects in making allowance for the color of the medium through which we look. So in the case of the colored shadow, which in reality is grey, we judge it to be blue—if we are using Helmholtz' method for producing them—because we think that we see it through a reddish-yellow medium. "When we have once judged it blue, if we make use of our tube, there is no reason for us to change our decision; nor is there any more reason to change it if the colored light is removed, since we observe no change of condition. But when colored light and tube are both removed, we judge at once that the shadow is grey, because we believe that we see it through a white medium, and when we replace our tube before the eye we continue to judge it grey, whatever change is made in the color of the light, because again we observe no variation of condition such as would cause us to modify our judgment. It is then proved, we believe, that the judgment of color rests not only upon a special property of the visual sensorial substance, but also upon unconscious anterior judgments, which have become so through habit, or because they depend upon instinct."[2]

Now let us examine these experiments more closely, and see if we cannot explain the facts quite as well by a purely physiological theory. For these experiments the following arrangements yield the most satisfactory results. In the shutter of a dark room is an opening, which, being furnished with a pane of colored glass, allows only colored light to enter. At this opening is fixed a small shutter, so arranged that it can be closed by means of a cord reaching to the table on which the shadows are cast, and can open itself automatically when the cord is released. White light is obtained through an opening some distance removed from the first, or from a gas jet or lamp. An opaque object on the table throws two broad shadows on to a white ground. Suppose now that we place a pane of green glass at the opening. The shadow cast by the white light will be strongly illuminated by the green light, and will be of that color. The general ground, illuminated by both lights equally, will be of a whitish green. The second shadow will appear to be of a color complimentary to this, a strong red, although it is illuminated only by white light. It is with this latter shadow alone that we are concerned.

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[1] Delbœuf, "La Psychologie comme Science naturelle," p. 58.

[2] Ibid, p. 61ff.

First let us see if, as Helmholtz says, we have to do with contrast effects only, and not with retinal fatigue. For this purpose we arrange our apparatus with the green glass at the opening, and have upon the table a tube so fixed to an iron standard that its upper end is stationary, while its lower end can be moved about at will. Let the shadow fall upon the paper, and mark a point within it at a little distance from its margin. Then arrange the tube in such a way that by looking through it you can see a portion of the shadow, including the point marked and also a portion of the colored field adjoining. Now by means of the cord close the small shutter, thus excluding the green light, rest the eye thoroughly, and then apply it to the tube. Carefully fixate the point marked, keeping the eye entirely from wandering, and open the shutter. A portion of the field visible will at once appear green, from the green light falling upon it. The shadow will appear of a slightly red tinge, with a tendency to decrease in intensity. This red is evidently purely an effect of contrast with the green, if the eye has been kept rigidly fixed. If the tube is now so moved as to cover the shadow only, and a new point is at once fixated, the red does not last for an instant on that portion of the retina on which the image of the shadow had fallen, but rather gives place to green or grey; while the other half of the retina, on which green light alone has worked, does experience the sensation of red, for it has been exposed to green-fatigue. This result follows clearly only under the most favorable circumstances. The difficulty of steady fixation, and of renewing the fixation after moving the tube, make the experiment difficult of success. The result varies somewhat also according to the length of time of fixation, to the size of the tube, to the amount of diffused light reflected from its sides, to the strength of the colored light and vividness of the shadow, to the care with which the eye is kept wide open, etc. Of course the various phenomena which accompany steady fixation, —the periodical disappearance and reappearance of the field, etc,— do not enter into consideration. But one result is always certain, if the experiment be performed with care,—the red of the shadow does not persist. The following modification of this experiment is perhaps less liable to failure on account of the difficulties mentioned above. After fixating the marked point in the red shadow close the shutter, thus excluding the green light, without moving the tube. Now the green almost surely gives place to red, and the red to a faint green.

This experiment gives us a very important result: the red color of the shadow, which Helmholtz found so lasting even when the green light was shut off, is not purely a contrast red; for the pure contrast red, as we have just seen, always disappears at once when the contrasting field is removed. The red which persists when the green light is removed is only the red induced when the eye has been allowed to wander over the field, or when it has been already somewhat exposed to the green light pervading the room, and when therefore the color is due not to contrast alone, but also to the effect of green-fatigue.

All these phenomena admit of an easy physiological explanation, but directly contradict the psychological claim, namely, that the red of the shadow, even when due to contrast alone, persists when the neighboring green field is excluded. Let us, for example, consider the explanation according to Hering's physiological theory. He maintains that the excitation of any portion of the retina induces

a stimulation of the neighboring parts, tending to produce in them the sensation of the complementary color; this being the physiological cause of simultaneous contrast. This would account for the color of the shadow in the original fixation. The continued action of the green on one portion of the retina would produce green-fatigue, by diminishing the green-red visual substance (or by building it up above the normal, according as green is the dissimilative or the assimilative color.) The red process on the other portion of the retina would produce the opposite effect, giving rise to a slight red-fatigue. On suppressing the external color-stimulus, (by closing the shutter or by moving the tube,) each portion of the retina tends to return to its normal condition, with the effect that the green yields to red and the red to green.

Having now learned that the red when it persists cannot be merely a contrast red, but must be an effect of green-fatigue, perhaps we can explain the phenomena which Helmholtz describes, in accordance with the physiological theory. Helmholtz' first experiment, which we quoted above, we make as follows: Let first the shutter be closed, excluding green light, and let the movable tube be so fixed that it is directed upon the subsequent position of the shadow, without including any of the neighboring field. Look now with one eye through the tube, the other being closed. The ground will appear grey. If now, with the eye still at the tube, the shutter be opened, causing the shadow, no change of color will be observed, but merely a slight one of illumination, or perhaps a somewhat greenish tint due to diffused light. This latter will yield to a faint negative (fatigue-induced) red on closing the shutter, lasting for a moment only. The shadow under these circumstances naturally does not appear red; for the physiological cause of the redness is not present, in that there has been no exposure of the retina to the green light of the field.

To one not looking through the tube, however, when the shutter is opened, the shadow at once appears red and the ground a whitish green, which rapidly becomes weakened in intensity. The latter fact is due to the diminished sensitiveness to green rays, on account of retinal fatigue; or, in Hering's language, the exposure of the retina to green light rapidly modifies the green-red visual substance, diminishing the sensitiveness to green and increasing the sensitiveness to red; the stimulus from the green rays remaining constant, the weight of the green element in the total sensation rapidly tends to decrease, of the red element to increase; hence results as total sensation a much lighter green. As the green becomes less intense, the red of the shadow becomes more so, for the same reason.

If now we look again through the tube, first at the shadow with a small portion of the general field at its edge, then at the shadow only, the shadow continues to appear red. For the retina, moving about over the green field, has suffered green-fatigue. Therefore when the green stimuli cease, the tendency is to restore the green-red substance to its normal condition, and the red process results. This condition now persists, even when the shutter is closed, for the color is due not to the objective color of the shadow, nor to the judgment, but to the physiological condition of the retina; and it will persist until equilibrium is restored. That this restoration of equilibrium does gradually take place is a fact overlooked by Helmholtz; but we can easily convince ourselves of it by experiment. After directing the tube onto the shadow alone, with the shutter

open or closed, we observe by patient watching that the red color slowly disappears until finally the field appears white or grey. Von Kries, who supports the psychological explanation, yet acknowledges that the "color does not last indefinitely long, but soon becomes doubtful."[1] The same fact has been observed by several persons who have performed the experiment with me, although they had no suggestion of what would probably occur. This is exactly the result to be expected if we accept the physiological explanation, as we have seen. But from the psychological standpoint it is hard to see why the red color should disappear at all, so long as we continue to look through the tube. For, as Delbœuf says in the passage quoted above, there is no reason to change our decision, if the color is due to an "unconscious anterior judgment," for the observable conditions do not change. We have therefore already strong reasons to doubt the assertion of Helmholtz that the red color is one which persists "after the circumstances which caused it have disappeared" owing to continued "deception of judgment."

The length of time during which the red color persists varies greatly, and sometimes the disappearance is very slow. It seems to bear some relation to the strength of the green light, and the length of time that the eye has been exposed to it. If, while the red still persists, one lay aside the tube and glance at the field, (the shutter being closed or the colored glass removed,) the color at once disappears; for it is thrust "under the threshold" by the stronger sensations coming from the field. But one can often see it again when these stronger sensations cease on applying the eye again to the tube. If one continue however to gaze through the tube alone after obtaining the red color, there are no neighboring active fields to influence it and no rival processes to interfere, and the change is sometimes very slow. A subsequent experiment will illustrate another case where under similar conditions an after-image is long retained, showing that the slowness with which the equilibrium is restored in this experiment is by no means exceptional.

When the sensation of red has become weak, there occurs a doubtful stage, where the phenomena are liable to vary in different observers. For example, while still watching at the tube with the shutter closed, if the other (unfatigued) eye look through a second tube at the white ground, and its field be compared with that of the observing eye, the red of the latter will sometimes seem to disappear suddenly, and the field is seen to be white. Von Kries speaks also of being able, at this stage (where the process has become very weak), to hasten or delay the change, and see the color almost at will red or white. This appears to belong to that class of cases where, the sensation being weak, it is often doubtful whether it exists or not. We often cannot tell whether we hear or imagine a faint sound, and sometimes under favorable circumstances we think that a musical note still continues, even after it has ceased. So here, at this doubtful stage, the sensation may be imagined for a short time after it ceases, and then disappear on comparing it with the white field. This says nothing however against the original red having been due to actual physiological processes. Quite as often, when one thinks the red color entirely gone, it will again be seen if the field seen through the original tube be compared with the grey field through the second tube. Again, after long gazing through the tube, there

[1] Gesichtsempfindungen, p. 132.

is always a darkening of the field underneath, due to fatigue of the black-white substance, and at its edges more or less contrast with the black sides of the tube. This effect is liable sometimes to be taken for a weak sensation of color.

These are all cases belonging to a large class of judgments where very weak nervous processes answering to sensations and nervous processes answering to pure mental images, are indistinguishable from one another, and where weak impressions are easily misinterpreted.[1] But they have not necessarily anything to do with color-contrast and with the cause of the shadows which we are considering.

We have thus reviewed all the "crucial" experiments of Helmholtz, and found them valueless as proofs of the psychological nature of color-contrast. A few further experiments are of interest, and easily admit of a physiological explanation. For instance, if, laying aside the tube, we steadily fixate for a considerable time one point of the middle of the shadow, the red becomes weaker and finally almost disappears, leaving the shadow greyish except at the edges, where the contrast is more marked. This is because the fixation is favorable to the restoration of the equilibrium disturbed by green-fatigue. If again we fixate the shadow, and then after a considerable time shut off the green light, the rest of the retina is subject to a bright red sensation, due to the restoration of the exhausted green-red substance; and the portion corresponding to the shadow experiences the sensation of green, due largely to contrast with the red.

Finally, the following experiment, to which we have already referred, illustrates the slow disappearance of an after-image under favorable circumstances, and thus shows that the same phenomenon in our tube experiment is not exceptional. Admit only white light to the room, then look for a time through the tube at a colored field (e. g., a piece of red paper); then move the tube so as to cover a white or grey ground, or remove the colored paper. The color complementary to the original field (in this case green) will be the color now seen, and not the original color (red), nor white or grey. And this color remains visible for a long period of time, as the red did in the shadow experiment. In the shadow experiment, after gazing at an apparently colored field (the shadow), the same color continues to be seen in the tube on a gray ground; in this experiment the complementary color. But the same law operates in both cases; for the two colored fields observed are due to different causes. In the first, the red is due, not to objective, but to subjective stimuli. The continuance of this red produces no red-exhaustion, but the gradually disappearing green-exhaustion merely continues, and is present so long as the red color continues as its sign. In this second experiment the color is objective, and produces exhaustion of that color, leading to a restoration of equilibrium which produces the sensation of its complementary color.

[1] On these cases, Prof. Sully, "Illusions." (Internat. Sci. Series), p. 35: "It is a law of sensory stimulation that an impression persists for an appreciable time after the cessation of the action of the stimulus. This 'after-sensation' will clearly lead to illusion, in so far as we tend to think of the stimulus as still at work."—p. 41: "It is evident that all indistinct impressions are liable to be wrongly classed. Sensations answering to a given color or form are, when faint, easily confused with other sensations, and so an opening occurs for illusion."—p. 39: "It is possible for the quality of an impression, as, for example, of a sensation of color, to be appreciably modified when there is a strong tendency to regard it in one particular way."

By the above experiments we have endeavored to prove the invalidity of the claims that colored shadows must necessarily be explained as an effect of deception of judgment, and that they are therefore a proof of the psychological nature of color contrast. We leave it still an open question whether contrast is psychological or physiological in its nature. Hering's formulæ seem to apply to it as well as do those of Helmholtz. Colored shadows throw no light on the controversy; for the experiments with colored shadows, which have been supposed to prove conclusively the psychological theory, are experiments not in color-contrast but in retinal fatigue. The unwarranted claims of the "psychologists" have resulted from the fact that hitherto care has not been taken to distinguish between the two phases of the shadow—that of pure contrast, which ordinarily is never seen, and that due to fatigue by the prevailing light, which begins at once over the entire retina, since the eye keeps moving continually over the field. Helmholtz waited for this effect before performing his tube experiments, as we notice by his direction to allow the blue of the shadow first to become intensely developed; and then he wrongly called the result an effect of contrast. His followers have done the same. Our positive result, besides the separation of the two phases of the shadow, is this: We have seen that the color of the shadow persists in the experiments with the tube only when it is due to retinal fatigue; that its long continuance is paralleled by an experiment in which the persisting color is an after-image which is not caused by "unconscious anterior judgments" induced by the belief that we gaze through colored media; that the color gradually disappears, a fact which cannot be accounted for by the psychological theory; that when it disappears suddenly by comparison with the grey field outside, it is because the sensation is thrust under the threshold, or has reached the stage where it is more imagined than seen; and that all the other phenomena observed are entirely compatible with a physiological explanation—some of them being thus even better accounted for. These facts, we believe, entirely disprove the claim that only a psychological explanation of colored shadows is possible, and make some physiological explanation at least possible—perhaps even absolutely necessary.

EDMUND B. DELABARRE.

Ueber den Grund der Abweichungen von dem Weber'schen Gesetz bei Lichtempfindungen. H. EBBINGHAUS. Archiv f. d. ges. Phys. Bd. XLV.

The researches of König and Brodhun, more rigid than any that have hitherto been made (this JOURNAL, Vol. II, p. 330), as to the degree of exactness with which Weber's Law applies to sensations of light-intensity, over a wide range of variation of intensity, and for six different points of the color scale, have induced Ebbinghaus to endeavor to account for the fact that departure from the law is the rule, and that its exact applicability holds only over a small range of medium intensities. Without attempting any explanation of the fact that the intensity of a sensation increases as the logarithm of the intensity of its objective cause, he endeavors to show why that should *not* be the case at the two ends of the intensity-scale, and in the following way. Assuming that the process which goes on in the retina is of a chemical nature, it is necessary to apply to it the conceptions that are current in modern chemical theory. The chemist has given

up the idea with which he started out, of the absolute uniformity of the substances with which he deals. As the physicist now identifies the pressure of a gas confined within a given volume with the *average* energy of motion of particles which are flying about with countless different velocities, so the chemist is now forced to look upon a given chemical constitution, not as a fixed and homogeneous, but as a *mean* state of aggregation of a given lot of atoms or groups of atoms. (B. Horstmann, *Theoretische Chemie*). When any agent is applied whose effect is to produce increased dissociation, it will have a fixed *average* effect upon the whole mass of matter concerned, but its actual effect upon any individual molecule will depend upon the condition of that molecule before the new force was applied. If it was already so *loose* in structure (to use a word for which there seems to be an absolute necessity), that is, if the atoms which composed it were so remote from their mean center and performing such rapid oscillations of their own that the force applied was just sufficient (or more than sufficient) to break its bonds altogether, then that molecule would be destroyed. A molecule which was of firmer constitution would not be destroyed by that particular application of energy, but would simply be brought nearer to a state of dissolution than it was before. If, now, the disgregation conditions of the different molecules occurred in accordance with the usual law for distribution about a mean, much the greatest number of the molecules would be in a state of disgregation near the mean, a gradually diminishing number would be of looser, and also a gradually diminishing number of a less loose constitution. Consequently, the effectiveness of any applied force (the light falling upon the retina in the case before us) would be greatest if it were just capable of breaking up the molecules near the mean. If it were of a less intensity there would be a relatively smaller number of molecules which it was capable of destroying; if its intensity were increased, there would again be a relatively smaller number of molecules left to be destroyed. This agrees in sense with the departures from Weber's Law; a low intensity and a high intensity of light are both less effective than that law would require them to be. Ebbinghaus attempts to show that there is a very exact correspondence between the departures exhibited by König and Brodhun, and those required by his theory. But in determining the actual distribution of disgregation states among the molecules, he transforms the kinetic energy of each atom into potential energy and then counts up the influences which affect, not its actual kinetic energy but its equivalent energy of position. This latter proceeding is plainly unjustifiable. The coincidence which he obtains between theory and fact seems to be, nevertheless, remarkably close.

It is a very curious proceeding in scientific method to attempt to account for the deviations from a law, when there is absolutely no explanation of the law itself—no physical explanation, that is; Mach's explanation is of a teleological nature. It seems to us to be, however, in the present instance, a proceeding which is not wholly illegitimate. The explanation itself rests, of course, upon the assumption that the relativity of Weber's Law is an objective fact, and not an affair of the judgment. C. L. F.

Beiträge zur experimentellen Psychologie. HUGO MÜNSTERBERG. Heft 1. Freiburg (J. C. B. Mohr) 1889. 8vo. pp. 188. 4 mks.

Dr. Münsterberg, a docent in philosophy at the University of Freiburg, announces a series of publications (of which this is the first installment) similar to the studies from Wundt's laboratory, with the exception that Dr. Münsterberg personally takes part in all the experiments and himself writes up the results. He hopes to publish about three numbers a year, and the new adventure certainly deserves a hearty encouragement, especially considering that the laboratory in which the studies are made is a private one without governmental endowment. It may as well be said first as last that the chief defects of these contributions (a defect perhaps equally prominent in Wundt's *Studien*) is the undue prolixity of the treatment, that so buries the real objects of worth as to give almost as much credit to the one who digs successfully into the heap as to him to whom it owes its origin. The complete results obtained by Dr. Münsterberg could have been forcibly stated in a maximum of seventy-five pages, with at the least doubling the number of his readers, and increasing ten-fold the appreciation of his work. It would perhaps be irrelevant to emphasize this point were it not that psychology has suffered from this weakness so severely in the past and will not gain its proper place amongst the sciences until it gives its results a truly scientific form.

Of the two studies here presented the first states the author's position on the general relations of body and mind, while the second gives an account of some ingenious experiments upon the time-relations of mental phenomena under the misleading title of "conclous and unconclous combinations of perceptions (*Vorstellungsverbindungen*)." The former is a somewhat complicated statement of the modern "psychophysic" view of the mutual influence of physical and psychical conditions with special reference to the factors of apperception and consciousness in the process. The train of thought is much broken into by digression (particularly of a controversial nature against Wundt) and is only indirectly concerned with the experiments, although the author tells us that his guiding *motif* was to throw light on the relation of the subject that watches and records mental operations to that other subject that in part with, and in part without consciousness elaborates them.

The experimental contributions fall into two parts, the facts brought out in them being the following:— (1) Following out the distinction established by Lange between a "sensory" mode of reaction (in which the attention is concentrated upon the expected stimulus) and the "motor" form (in which the attention is fixed upon the intended movement), Dr. Münsterberg measures the reaction-time to a sound with each of the five fingers with each form of reaction, and finds on the average a sensory time of 162 σ and a motor time of 120 σ (σ = .001 second), a difference of 42 σ, considerably less than Lange's (100 σ). (2) Next, the words one, two, three, four, five were called out irregularly, and the thumb, forefinger, etc., were to be raised when the corresponding number was called. Sensory time, 383 σ; motor, 289 σ. (3) The process remains the same, but the Latin declension *lupus, lupi, lupo, lupum, lupe*, is substituted for the number words, evidently a more artificial association. Sensory time, 465 σ; motor, 355 σ. (4) The movements of the five fingers are associated with the cases of the three following German pronouns, *ich, meiner, mir, mich, wir*; or *du, deiner, dir, dich, ihr*; or *der, des, dem, den,*

die, the same reaction following to any one of three stimuli. Sensory time, 688 σ; motor, 430 σ. Errors, *i. e.*, raising the wrong finger practically never occurred up to this time; from here on they occur, but only in the motor reactions, and always consist in the raising of a neighboring finger. In the present case the errors form 10 per cent. of the reactions; the second finger being frequently raised to *du* apparently from its place in the series, *ich, du, er*. (5) The process becomes more general; if a noun is called, the thumb is to respond; if an adjective, the next finger; if a pronoun, the next; if a number, the next; and if a verb, the little finger. The words are all monosyllabic; no word occurs twice, and the general association of parts of speech with fingers is well learned in advance. Sensory time, 712 σ; motor, 432 σ; errors, 30 per cent. (6) The process remains the same, the categories being respectively, a city, a river, an animal, a plant, an element. Sensory time, 893 σ; motor, 432 σ; errors, 12 per cent. (7). The categories are an author, a musician, a naturalist, a philosopher, a statesman or general. Sensory time, 1122 σ; motor, 437 σ; errors, 25 per cent.

This remarkable and rapidly increasing difference between a sensory and a motor reaction demands a more complete explanation in the light of the facts just presented. After spending a needlessly long argument in showing that Wundt's apperception theory fails to explain the facts, Dr. Münsterberg concludes that in the sensory apperception we attend to one step at a time, allowing each factor to pass through the mind *seriatim*, (and thus sensory reactions remain a trustworthy test of mental complexity); but that in the motor times we have a distinct overlapping of mental processes, the mechanism being a short circuit between the finger-moving centers and the particular word called, the processes of recognizing the word as belonging to a certain category, and that category as associated with a certain finger not emerging in consciousness and being performed simultaneously with the other process. This process would be about the same in cases (4) to (7) and these give about the same motor, through vastly different sensory times. In other words, motor complexity is a different matter from sensory complexity, and a comparison of the gradual increase of each time from (1) to (7) will reveal many suggestive points, with reference to which every theory of association or perception must be built.

The second study is upon the association of ideas. There are two subjects, *M* and *R*, who react by pressing a key in pronouncing their reply-word, while Dr. Münsterberg had pressed the key in speaking the last syllable of the call-word or the call-sentence. Besides the times the average variation (*v*) is given.

(1) Simple repetition of the call-word occupies *M*, 403 σ (*v*. 60 σ) and *R*, 362 σ (*v*. 70 σ). (2) Ordinary association-time for *M*, 845 σ (*v*. 140 σ); *R* 948 σ (*v*. 170 σ). (3) A limited association, the general type being, "Given a general term to name an instance under it." The kind of relation was constantly varied, such as, "A German wine, Rudesheimer," "A Greek poet, Homer," etc. *M*, 970 σ (*v*. 200 σ); *R*, 1103 σ (*v*. 210 σ). (4) An association limited to a single answer, after the pattern of question and answer: *e. g.*, "On what river is Cologne? Rhine." "In what season is June? Summer." *M*, 808 σ (*v*. 180 σ); *R*, 889 σ (*v*. 140 σ). (5) The answer is now the result of comparison and judgment; e. g., "Which is larger, a lion or mouse?" "Who is greater, Homer or Kant?" *M*. 906 σ (*v*. 180 σ); *R*, 1079 σ (*v*. 220 σ). (6) Same as (5), but each query is preceded by a series

of about a dozen words of the same category as the terms compared. Thus: "Apples, pears, cherries, nuts, peaches, grapes, strawberries, dates, figs, raisins,—which do you like better, grapes or cherries? Cherries." *M*, 684 σ (*v.* 130 σ); *R*, 659 σ (*v.* 160 σ),—a shortening of 212 σ and 460 σ as compared with (5). (7) A combination of (3) and (6); instead of first asking for a drama of Goethe's, and then asking which is the finest, this, that or the other, we ask at once, "Which is the finest drama of Goethe?" *M*, 962 σ (*v.* 180) σ; *R*, 1137 σ (*v.* 160 σ). (8) This is a comparison between a given term and a term derived by process (7); thus, "Which is the more westerly, Berlin or the most important German river? Rhine." *M*, 1844 σ (*v.* 370 σ); *R*, 1866 σ (*v.* 340 σ). (9) Bears the same relation to (8) that (7) does to (4); the type of query being, "Which lies more westerly, Berlin or the river on which Cologne is situated?" *M*, 1291 σ (*v.* 180 σ); *R*, 1537 σ (*v.* 230) or 553 σ and 529 σ less than (8) while (7) is only 154 σ, 248 σ less than (4). (10) is (9) preceded by a series of terms of the same category as those to be compared. *M*, 1153 σ (*v.* 170); *R*, 1145 σ (*v.* 210), or 138 σ and 192 σ less than (9). The results are based upon 890 experiments in all.

After again fiercely combatting Wundt's apperception theory in its relations to these results, Dr. Münsterberg attempts their interpretation, in which the four following relations may be selected as most important: (*a*) that the time of a free association is shorter than of a limited association, (2) shorter than (3); (*b*) the question-answer association is shorter than the limited, (4) than (3); (*c*) that the reading of the series of words before the question shortens the time, (6) is shorter than (5) and (10) than (9); (*d*) the combination of any two or three factors in the same process takes less time than the sum of the times needed for each of the factors separately; compare (9) with (3) and (5). Relation (*a*) is not new and is readily explained by considering that in one case *any* association aroused by the call-word will answer, while in the other case several associations may arise that must be rejected. Fact (*b*) seems to indicate that we do not call up first the general notion and then the particular, but seem to have a direct and usually prominent association with the particular, the irrelevant general association not being "apperceived." The third fact indicates the mechanism of preparation, certain general lines of association being already ruled out by the formation of the category-series, and thus less of the mental labor comes into the measured time. The fourth circumstance shows again the overlapping of mental processes; the mind is not a point through which each process must pass in turn, but is a plane in which the most complex interactions have play. For the acceptance and further development of the interesting results of these studies we must look to the future J. J.

L'Énergie et la Vitesse des Mouvements Volontaires. CH. FÉRÉ. Revue Philosophique. Juillet, 1889.

In this paper Dr. Féré gathers up the results of a number of brief studies, for the most part reported during the past year in the *Comptes rendus de la Société de Biologie.* Beginning with pathological subjects, he shows that the reaction-time of hysterical hemianæsthesics is generally longer than normal (and, as some of his experiments seem to show, the rate of nervous transmission on the diseased side slower) when the diseased side has part in the pro-

cess, the lengthening corresponding in general to the diminution of sensibility and muscular power. These changes in force and rapidity depend upon differences in nutrition which are in part demonstrable. The plethysmograph shows an increase of the volume of blood in the hand preceding the voluntary reaction, apparently one of its prerequisites. Other experiments show that the electrical resistance is less, and that the reduction of oxyhemoglobin is slower upon the anæsthesic side. The reaction-times and the reduction of oxyhemoglobin are strongly influenced by sense stimulation and by emotion, either natural or hypnotically suggested. Experiments were also made upon epileptics with reference to the quickness of the response before and after the paroxysm, the last being slower and corresponding to the anæsthesias and weakness which seem, in some degree at least, always present. The slowing does not seem to depend on the frequency of the attacks; it serves, however, to show the persistence of the stupor. Here also slow reactions accompany diminution of oxyhemoglobin, a fact that the author uses in defending the fatigue theory of post-epileptic paralyses.

Experiments on normal subjects show the same general relations between muscular vigor and quickness of response. With a new dynamometer of varied applicability, Féré has been able to compare the powers of extension and flexion of the different fingers among themselves and with their reaction-times. The following table shows the results in a single case:—

	Flexion.		Extension.	
	Dynamometer (Kilograms.)	Reaction-times (Seconds.)	Dynamometer (Kilograms.)	Reaction-times (Seconds.)
Thumb	4.200	0.163	1.200	0.19
Index finger	4.900	0.191	1.000	0.261
Middle "	3.500	0.168	.900	0.28
Ring "	2.000	0.201	.600	0.299
Little "	1.900	0.203	.400	0.31

A similar table from a pianist shows quicker reactions and greater strength in the last three fingers. In right-handed subjects the reaction-time is a little quicker on the right than the left, even when the reaction is made to the same stimulus by both, (in which case both are a little slower): *e. g.* (flexion) right hand alone, 0.12 sec.; left hand alone, 0.16 sec.; both hands together, right 0.14, left 0.18. Some subjects reversed the last relation, and developed more force and reacted more quickly with both hands; they were, however, rather defective epileptics. Normally it would seem that the "rapidity and abundance of the nervous avalanche are by so much the greater as the ways of discharge are less numerous." In the lower extremities which act most powerfully in extension, the extensors give the shortest reaction-times. Normal subjects give quicker and more forcible propulsions of the tongue than deaf-mutes and stutterers. The effect of warming the hand in hot water is to quicken reaction, the greatest change appearing in the fingers reacting most slowly before. Féré's experiments make an impression of care and judgment as compared with those of Rémond, reviewed in a previous number (Am. Jour. Psy., II, 486). They were, however, executed in part with the same questionable instrument, but in part also graphically with Marey's pneumatic drums. The number of observations going to a series were apparently small, and mean variations are never given—a serious omission if any intelligent judgment is to be passed upon the results.

Experimentelle Studien zur Individualpsychologie. AXEL OEHRN. Inaug. Dis. Dorpat, 1889.

Simplification is a thing much to be desired in psychic measurements, and until such can be secured, their application which might be considerable must remain comparatively limited. The point of Oehrn's thesis is the demonstration of the applicability of a few simple methods—not all new, but never before applied on quite this scale. His subjects counted letters from the printed page, added single syllabled digits to 100, wrote at dictation and read through successive periods of 5 minutes, and committed to memory numbers and meaningless syllables, the experiments generally lasting 2 hours and the subject working at the top of his speed. These processes, though more complex than those measured in reaction-time experiments, are simple enough to lead to interesting and rather regular results. The following table shows the average time on ten subjects in thousandths of a second for a single stage of the different tasks, *e. g.* counting one letter, reading one syllable, etc.:—

	Average Time in 0.001's Sec.	Max.	Min.	Average of Mean Variations in per cents.
Counting single letters,	406	530	317	4.2
" letters by threes,	323	440	209	5.0
Adding pairs of numbers,	1244	1583	754	4.6
Writing single letters,	435	603	331	2.6
Reading single syllables,	133	172	116	3.4

	Average Time in Seconds.	Max.	Min.	Average of Mean Variations in per cents.
Learning 12-place numbers,	9.9	20	4.2	14.7
" 12 nonsense syllables,	11.8	21.4	7.33	27.4

The author's analysis of these processes and the comparison of his results with those of Cattell, Berger, Dehio and others, must be consulted in the original. Of more importance, however, than these absolute results are the relative ones which bear upon attention and the effects of practice and fatigue. The former is studied in the mean variation which Buccola has aptly called "the dynamometer of attention." Reading and writing, for example (see table above,) are almost automatic in educated persons, while the learning of nonsense syllables requires the most vigorous voluntary attention. By comparing the amount of work accomplished in successive periods during the two hours the effects of practice and fatigue are discovered. The typical curve shows an ascending phase in which practice more than balances fatigue, quickly followed in general by a descending phase in which fatigue more than balances practice. From this there are not a few variations, mostly explicable by changes in the three factors (attention, practice and fatigue) from which the curve results. Fatigue announces itself by irregularities even before the summit of the curve, but these again disappear as the subject becomes too tired even to "spurt." The relative gain through practice is less and less as practice increases, as has before been shown by the experiments of Guicciardi and Cionini. The last chapter of the pamphlet is devoted to individual differences, and though several are clearly demonstrated, the number of persons studied was too small for generalization. Among other suggestive points may be mentioned the evidence for a 24-hour periodicity in capacity for intellectual work; of the two persons tested one was at his best in the evening, the other in the morning.

Versuche über die Ablenkung der Aufmerksamkeit. ARVED BARTELS. Inaug. Dis. Dorpat, 1889.

The writer of this thesis, like Oehrn, a pupil of Kræpelin's, has studied the distraction of the attention in the field of light sensations by finding the variation in the threshold stimulus for one eye, when its reception was preceded at a short interval by another stimulus in the other eye. For the description of the rather complex apparatus and the detail of the experimentation the reader must be referred to the original; the following points, however, may be mentioned: the threshold was fixed at the point where half the judgments were correct (method of right and wrong cases); the distracting stimulus was 1515 times as intense as that of the threshold, and lasted 0.171 sec. against 0.448 sec. for the latter; the intervals between the two were varied from 0.1 to 10 seconds, those from 1 to 6 receiving most attention. Evidence of distraction was found, lasting as long as 6 seconds, but evidence also of a diametrically opposite effect, the intended distraction acting like the warning signal in reaction-time experiments and helping to concentrate the attention. The intervals at which this was most marked were about 2¾ sec. and 4¾ sec. with some signs of another such period at a little more than 7 seconds. This rhythmic change recalls those found by time-sense experimenters, but is much longer (Estel 0.75 sec.; Mehner 1.4 sec.; Glass 1.25 sec.). It agrees, however, pretty well with the interval in estimating which the smallest error is made, and with this it is probably connected; L. Lange, Ewald and Wundt have found the two-second warning to be the most effective one in reaction-time experiments. The experiments tried show that there is no connection with the two-second periodicity of the unconstrained attention (N. Lange). When the interval between the stimuli varied each time and the subject only knew the limits within which it might vary, he seemed to adjust his attention to a point midway between.

Ueber die psychologischen Grundlagen der Vergleichung gehobener Gewichte. G. E. MÜLLER und FR. SCHUMANN. Reprint from Pflüger's Archiv., Bd. XLV, pp. 37–112, 1889.

This very important paper is an investigation of the simple illusion which makes a light weight lifted after a heavy one seem disproportionately light, and *vice versa*. The importance for psychophysic experimentation of such a study is obvious, but the writers value their work less as a contribution to that subject than to the theory of motor adjustment and organic memory. The weights were lifted behind the back with every precaution to secure accurate results. A typical experiment is as follows: A standard weight of 676 grammes was compared five times each with weights of 626, 676, 726, 776, 826, 876 grammes. The last was recognized as heavier every time, the next-to-the-last four times. The standard weight was then compared 30 times with a weight of 2476 grammes, and after that once each with weights of 926, 876 and 826 grammes. All seemed lighter. After five more comparisons with the 2476 gramme weight, it was again compared with the last three, and so the experiment continued. In this case the standard and comparison weights were lifted with different hands, but the illusion occurs in single handed experiments also, and it is even possible in a certain degree to disturb the judg-

ments of weights lifted with one hand by lifting heavy weights with the other.

This illusion is due, not to "contrast," but to an unconscious adaptation (*Einstellung*) of the motor centers to the more vigorous impulses required by the heavier weights. In the normal comparison of weights the nervous discharge for the lifting of each, though wholly unknown in amount to the subject, is the same. *The judgment is made from the speed with which the weight comes up.* When by the change of weights a discharge adapted to a heavy one meets a light one, the latter rises with an unaccustomed velocity and is therefore pronounced lighter. Such motor adaptations may be of different kinds, and, corresponding to them, there is an adaptation of the sensory attention. Such adaptations play a part in ataxic phenomena: (*e. g.* a patient unable to move a given finger at request except with open eyes, once having developed a motor adjustment by moving it so, can thereby keep on moving it for a little while after his eyes are closed), in the pleasure of rhythmic movements, the periodicity of the time sense, etc. The authors reject the innervation sense *in toto*, (in a later chapter they subject it to a thoroughgoing critique), likewise the muscle-sense, at least as a factor in judging weights. If such senses are the discriminating ones, they should tell us of the amount of force expended and prevent the illusion. The theory that weights are judged by the velocity with which they are lifted was suggested by Hering in a letter to Fechner, and was adumbrated by Lewinski and others, but has now for the first time been given a full experimental treatment. If weights are judged by velocity, the question of the perception of the movements and positions of the limbs becomes a cardinal one. On this point the authors accept the views of Goldscheider, locating the sensations chiefly in the joints, but reject his notion of an independent motion-sense.

Other sources of error were investigated, *e. g.* the *Zeitfehler*, or error introduced by the order of lifting the weights and the time interval between the lifting of the standard and of the weight to be compared, and the *Raumfehler*, or error introduced by the different distance and direction of the weights from the body of the lifter. Both of these find interesting and plausible explanation in the adaptation theory stated above. Special fatigue experiments showed, contrary to Fechner's "parallel law," a decline in the discriminative sensibility. A third weight lifted between the two to be compared, at first caused large errors, but the subject soon became able to neglect it. Weber's law follows as a necessary consequence of this theory, provided that light and heavy weights are judged by their velocities, for the just observable difference is then a difference of velocities, and it is a principle of mechanics that the change in mass needed to produce a fixed (just observable) change in the rate of a moving mass must vary proportionally to that mass. Practically however the law is much overlaid by confusing circumstances.

Voluntary Control of the Heart. EDW. A. PEASE. Reprint from the Boston Med. and Surg. Journal, May 30, 1889.

Mr. Pease reports a set of experiments upon a student in the Harvard Medical School who possesses this rare power. The subject was able with little irregularity of respiration to increase his pulse-rate for a few seconds in the proportion of about 17 beats per

minute, and by holding his breath at the rate of 27 per minute. Other experiments show, however, that the power does not consist in any change in breathing, nor does it depend on increased blood pressure, voluntary motion, nor, according to the subject, on the fixation of any emotion or idea. The change of rate seems to be effected by a series of impulses which gradually weaken in force; the power to produce these is also easily exhausted. The subject, as seems general in such cases has a certain power over the ear muscles and others not commonly under control.

On the Observation of Sudden Phenomena. S. P. LANGLEI. Amer. Jour. of Sc. XXXVIII, 93, Aug. 1889.

Reference was made in an earlier number of this JOURNAL (II, 24) to a device of Prof. Langley's for excluding personal equation in transit observations. He now presents a simple and ingenious instrument for practically excluding it in the observation of sudden phenomena (*e. g.* the emergence of stars from the dark limb of the moon, etc.). The detail of the instrument must be seen in the original; but in general it depends on the introduction of a double total-reflection prism, revolving in the axis of the instrument. The image of the emerging star, or whatever the phenomenon be, is thus made to appear in a different sector of the field according to the part of the second in which it occurs. On trial with a field divided into 20 sectors and an artificial star, one of the observers, without special practice, reduced his probable error for a single observation to about one fortieth of a second.

Ueber die galvanischen Erscheinungen in der Haut des Menschen bei Reizungen der Sinnesorgane und bei verschiedenen Formen der psychischen Thätigkeit. J. TARCHANOFF. Pflüger's Archiv, Bd. XLVI. pp. 46-55.

These experiments were made with the galvanometer of Meissner and Meyerstein, the deflections of the mirror being read off on a scale by means of a telescope. The instrument was so sensitive that the current in the N. ischiacus of the frog was sufficient to cause the scale to disappear from sight. The electrodes were applied to different parts of the body, principally to the outer surface of the hand and the inner surface at the base of the fingers. The currents of a state of rest were compensated for, and the subject was, of course, undisturbed and motionless. It was found that tickling was sufficient to cause a strong deflection of the needle. After a latent period of from one to three seconds, the current was at first weak and slow, and then so strong as to put the scale out of sight. The inside of the hand was negative, the outside positive. Electricity, heat and cold, the prick of a needle, caused the same effect, but not to such an extent. So did stimulation of the special sense-organs, the sound of an electric bell, the smell of vinegar and ammonia, the taste of sugar, etc. After the eyes had been closed for some time, simply opening them was sufficient to produce a current. Different effects were produced by different colors,—it is not stated what colors were the most irritating. But when no sensation was experienced, the mere imagination of a sensation was sufficient to produce a change of from 10 to 15 divisions on the scale. The idea of extreme heat was especially effective, and still more so if the hand, which was being tested was

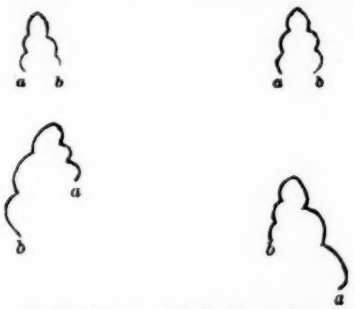
alone imagined to be in a state of perspiration. Any strong intellectual exertion produced a marked effect, even when the needle no longer responded to excitations of organs of sense; but a simple application of the multiplication table was not very effective. The effect of any contraction of a muscle was very marked. DuBois-Reymond first showed that a current is produced in the hand when the hand contracts, but he attributed it to the negative variation in the current in the muscle. It seems now that Herrmann is right in considering it to be a secretory current produced by the increased activity of the sweat-glands, for it is produced in every part of the body where there are glands, no matter where the muscular contraction takes place, and it cannot be detected in places where the sweat-glands are few or wanting. It is not so much the extent of the voluntary motion which regulates the amount of the current as it is the degree of conscious exertion that accompanies it; to fixate the point of the nose, makes a change of twenty divisions on the scale. If both points of attachment of the electrodes are well supplied with sweat-glands, the current is frequently found to move first in one direction and then in the other; if one only is so, the current is negative at that end. The existence of this current indicates an increased activity of the sweat-glands; it is well known that their activity develops the so-called secretory current. It is therefore proved that the course of nearly every kind of nervous activity,—from the simplest impressions and sensations, to voluntary motions and the highest forms of mental exertion,—is accompanied by an increased activity in the glands of the skin. The plethysmographic investigations of Mosso, François Frank and others show that the blood-vessels of the extremities take part in all kinds of nervous activity. But they contract, and hence they are not the cause of the increased activity of the sweat-glands. That must be due to a special excitation of the nerve-centers which regulate them. Its purpose is plain; all nervous activity means an increased accumulation of the products of decomposition and a rise of temperature; the greater activity of the sweat-glands facilitates the elimination of the products of decomposition, and at the same time serves to bring down the temperature.

C. L. F.

Untersuchungen über die Orientirung im Fühlraum der Hand und im Blickraum. J. Loeb. Pflüger's Archiv, XLVI, S. 1-46.

Loeb has made the important discovery that when we will to make equal excursions of the hand in the hand-space, the excursion actually executed is shorter than that willed, if the muscle concerned in making it is shorter than in a state of rest; and is longer, if the muscle is longer. The method of conducting the experiments was as follows. A person stands before a vertical table and makes with one hand, say the right, an excursion along a thread of definite length. At the same time he wills to move the left hand (which is not guided by a thread) over an exactly equal length: if this hand starts from a point higher up than the other and moves *upward*, it moves *less* than it should do; if it moves *downward*, it moves *more* than it should do. The more the starting-point of this hand is depressed (the hand which follows the string remaining at a medium height), the greater are its upward motions, the smaller are its downward motions; but the lower the starting-point, the longer are the muscles which raise the arm, the shorter are those which depress the arm. A simple experi-

ment, by which anyone can convince himself of the sense of the illusion, is this. If, with the eyes shut, one attempts to draw a leaf with each hand, the two figures are very much alike, if the two hands are symmetrically situated. But if one hand is raised and the other lowered. the up-strokes of the one hand and the down-strokes of the other will be too short, or none of the four leaf-sides are drawn of the size intended to be drawn and supposed to be drawn.



The drawing goes each time from *a* to *b*.

The same thing holds for horizontal motions. But if horizontal motions are performed with the hand raised or lowered (but not horizontally displaced), the illusion does not occur; the muscles which effect *horizontal* motions are not put out of their resting-position by raising or lowering the hand. The error is very considerable in amount; the motion executed may be only one third as great or three times as great as that supposed to be executed. That it is the actual length of the muscle and not the tension it is under which causes the illusion, is proved by showing that the illusion is not produced by attaching weights to the hand; intended movements are executed very accurately even when the hand is restrained by a weight of nearly 6 kg.

Loeb finds, in opposition to Dr. Stanley Hall (*Mind*, No. XXXIII.) that gravity is of no effect in producing illusions of this kind; Dr. Hall happened not to try his experiments with the variable hand low down in its space, as well as high up, or he would have noticed that the illusion works against gravity as well as with gravity.

The muscles of the eye exhibit the same phenomenon; a distance looked over seems shorter in any direction, the further away it is in that direction. In indirect vision, the same thing holds, but with less exactness, and this leads Loeb to the conviction that the local signs of the retina are simply inhibited impulses of the will. There seems to us to be a lapse of logic in this, as well as in all other statements of the same theory. Granted that we estimate the distance of an object seen indirectly by a consciousness of the strength of the innervation which would be necessary to make us look at it, how do we judge of the required strength of innervation? In other words, if the imagined impulse of the will is the cause of the feeling of distance, hat must itself have a cause in some preceding sensation,

and nothing at all has been done to show that it is the impulse, and not *the sensation which determines the impulse*, that rises into consciousness, and effects our knowledge of the position of the object.

Loeb considers himself to have absolutely proved, by these experiments, that our knowledge of the extent and direction of our voluntary motions depends upon the impulse of the will to perform the motion, and not upon the sensations excited in the active organ of the motion. The two assumptions which the validity of this proof requires are, that no change in the sensations conveyed by skin, joint, tendon or muscle is produced by an unusual position of the organ (that is, that *sensation* is not a function of the present condition of the elements which communicate it, while the excitability of a muscle is a function of its state of excitation), and—what Loeb frequently affirms—that the constant hand and the variable hand are both moved by one and the same impulse of the will. To the consciousness of the present writer there seems to be, when the experiment is performed, the following processes going on in the mind. One is at once aware that, since the two hands are to start together and to end together, an accurate length is to be gone over by the variable hand by giving it the right velocity. During the tracing of the line (if it is done a little slowly) there is a constant estimation of the correctness of the velocity already executed (that is, a comparison of it with the velocity of the other hand) and a corresponding decision to increase the velocity or to diminish it for the rest of the excursion. When the hand is extended, the line is executed wrong; but how can one be sure that that is because a right degree of innervation has a diminished effect upon a muscle already contracted, and not because there is an error in the estimation of the amount of contraction which takes place in a contracted muscle? (It would be interesting to compare the steadiness of the velocity of the variable hand with that of the constant hand.) Nor does the assumption that both hands are moved by a single impulse of the will (and therefore by equal impulses) seem to us to be tenable. One is conscious, very plainly when the positions of the two hands are not symmetrical, of a rapid moving of the attention from one hand to the other, and that is, of course, the same thing as a rapid alteration of impulses of the will. Moreover, the two hands have totally different tasks to perform; the right has to follow a thread and to stop when it gets to its end; the left has to execute a perfect copy of the motion of the other. It is impossible that two such different commands as this should be executed by a single impulse of the will, nor is there any anterior reason for supposing that the two impulses required are necessarily equal in amount. The experimental results of Loeb's investigation are extremely interesting, but they do not seem to us to bear out his far-reaching conclusions.

C. L. F.

Ueber das mechanische Latenzstadium des Gesammtmuskels. DR. W. COWL. Separat-Abdruck aus den Verhandl. d. physiol. Gesells. Berlin. 1889.

In view of the difference of the results of Yeo's study of the "latent period" (see review, AMER. JOUR. PSY. II, 488) from those of Gad and more recent observers, the author has undertaken a repetition of Gad's experiments using the same methods and apparatus, and confirms his results, concluding that the "latent period" is due (though

perhaps not solely) to a stretching of the muscle preliminary to its contraction. He prefaces the statement of his own work by a brief historical summary.

Ueber den Muskelsinn. GOLDSCHEIDER. Verhandl. d. physiol. Gesells. Berlin. Sitzung am 17 Mai, 1889.

The hypothesis of a sensation of motion, distinct from that of the positions beginning and ending the movement, is supported by the following considerations. 1. The sensation of motion becomes clearer as the rapidity of the movement increases, and attends movements of too short duration for the complicated processes of a judgment from the positions. 2. The just observable sensation of motion accompanies movements so very small that their limiting positions are probably indistinguishable. 3. Sensations of motion are clearly perceived before the direction of movement is clear. 4. The sensation of position can be temporarily removed by faradizing without destroying the sensation of motion.

In experiments on the lifting of weights it is well to use only single segments of the limb. The physiological conditions of the experiment are thus greatly simplified, with the result that in lifting the weight by a thread nothing of the sensation of encountering at some moment the resistance of an exterior heavy object is felt (a prominent sensation in lifting with more than one segment,) but only the more subjective sensation of heaviness (*Schwere-Empfindung*), of greater difficulty in executing the previously easy movement. This sensation of heaviness has its seat in the tendons; that of resistance, like that of motion, in the joints, and is called forth by the pressure of the joint surfaces upon one another. It suffers if there is motion in the joint at the same time. In lifting weights in the ordinary way both sensations are aroused.

As against the participation of an innervation sense in these judgments the following facts are adduced. 1. The sensation of weight is felt when the contraction of the muscle is produced by electrical stimulation or reflexly like the knee-jerk. 2. The limb may seem perfectly relaxed when it is still partly supported by muscular tension. 3. Movements may be made actively as well as passively which are too small to be perceived by the subject, and the limit of perceptible movements is raised by faradization in one case as in the other. 4. Innervation sensations do not mediate the sense of position, for that is almost entirely destroyed by faradization. 5. Certain illusions exist which should not be possible with an innervation sense. The consciousness of voluntary movement comes from the immediate succession of peripheral sensations of motion upon the genesis of the corresponding motor image. (The weight of evidence at present is very strongly against the existence of innervation sensations, and those whose theory of space perception involves them will have to bethink themselves of reconstruction. REV.)

Zur Frage der psychophysischen Messungen bei Geisteskranken. M. K. WALITZKAJA. Archiv f. klin. u. gerichtl. Psychiatrie v. Merschejewski. I, 17. Rev. by Kraepelin in the Allg. Zeitsch. f. Psychiatrie. Bd. XLVI, H. 2-3, S. 245*.

These experiments were made on 7 insane and, for comparison, on 5 sane subjects. In 4 of the 7 cases the diagnosis was general paralysis, in 2 progressive paralysis, in 1 paranoia. Simple reaction-

times, choice-times, association-times, etc., and the times for adding digits were measured to a total number of 18,000. A table of the mean times required for these processes is given, also a table in which the association-times are further classified. But the very grave defects of method, (those which Kraepelin enumerates touch the control of the apparatus, the making of the protocol and the presentation of the results) rob the work of scientific value. The conclusions at which the author arrives are, in general, that in the beginning of paralysis there is an increase of automatic mental activity (including association) together with weakened volition; at a later stage the automatic activity also fails by degrees and simple perception is difficult. In nearly complete remission the automatic activities return fully, the voluntary and intellectual only partially; when the disease becomes acute the automatic processes again become prominent, the volitional difficult.

These results are not so very different from those found in the same field by Madam M. K. Wallcka.

On some Facts of Binocular Vision. J. VENN. Mind. April, 1889.
"*On some Facts of Binocular Vision.*" J. H. HYSLOP. Mind. July, 1889.

Prof. Venn finds himself unable to get some of the experimental results brought forward by Dr. Hyslop in a previous paper, (reviewed, AMER. JOUR. PSY. II, 159.) . In his reply the latter shows these differences to be more apparent than real and by no means important to the central idea of his paper, which was a criticism of Wundt's innervation theory. The discussion demonstrates as Dr. Hyslop notices, the great complexity of such experiments and the large part which personal variations in skill may have in the results.

L'agrandissement des astres à l'horizon. G. LECHALAS. Revue Philosophique, Juillet, 1888.
Une association inséparable: l'agrandissement des astres à l'horizon. M. BLONDEL. Ibid., Nov., 1888.
Continuation of the discussion, Ibid., Déc., 1888 and Fév., 1889.

Why does the moon look large at the horizon? The current explanation is that it then seems large because it seems far away, and seems far away because many things intervene. The object of Lechalas's paper is to review this theory in the light of certain experiments of Stroobant's (*Bul. de l'Acad. de Belg.* VIII (1884), 719; X (1885), 315). The most important of these were three. (1) He projected an after-image (of the sun in this case) upon a wall at such a distance that the after-image seemed of the same size as the sun, and found that distance to be always about 48 m. Platesu, who seems to have originated the experiment, found 51 m. with the moon. This shows, Stroobant contends, that these bodies always appear at the same apparent distance, and is therefore a definite disproof of the distance theory. Lechalas objects that the moon really seems much further than 50 m., and Blondel shows in his paper that while the experiment shows the constancy of the retinal image, it does not show the constancy of the apparent distance, appearances being for the mind and not the eye. (2) The second experiment, the force of which Lechalas admits without abatement and to which he assigns a physiological explanation, was as follows: The experimenter produced near the ceiling of a perfectly dark room two

electric sparks 20 cm. distant from each other, and in a horizontal direction at an equal distance from the eyes two other sparks whose separation could be varied. He found that when both seemed equal this separation was only about four-fifths that of the pair on the ceiling, and that the illusion persisted when the observation was made lying down. This, however, would account for only about half the difference of the apparent size of the moon at the horizon and zenith, and Stroobant found that when the rising moon was observed at the zenith by a mirror set at 45° it yet appeared larger than when actually there, a difference which he attributed to the faintness of the light at the horizon, verging thus toward Berkeley's view. (3). His third experiment was upon this point; the light of a lantern thrown into the eyes of the observer reduced the apparant size of the moon, and certain laboratory experiments seemed confirmatory; with the latter, however, Lechalas is dissatisfied. On the whole he believes the current explanation valid in part, but needing the addition found in Stroobant's second experiment.

Blondel's explanation goes deeper and makes the whole thing, including Stroobant's second experiment, psychic. The eyes see the moon always of about the same size; the illusion enters when the imagination introduces the element of distance. And this is no such simple thing as the common theory makes it; it may be present when there are no intervening points, as when the moon rises over a wall. One element in it is the shape we give to the sky (a low arch) by reason of the great preponderance of experience in horizontal extension as compared with that in vertical. With this co-operates the seeming accessibility of the moon when red and low and apparently in our atmosphere, "for then the tactile image supplants the visual image." Granted the apparent difference of distance, the false perception results from a piece of sensory logic: Of two objects which have the same visual angle, the further is the larger; the moon is further at the horizon than at the zenith; therefore, she is larger at the horizon. It is this aspect of the phenomenon that interests Blondel chiefly, and he goes on to show that the elements of these perceptions-by-inference are dependent on circumstances, (a red moon at the horizon seems large, but one reddened in eclipse seems small); "alone, each detail explains nothing; there must be grouping and generalization, in order that a perception result from it, as the conclusion results from premises of which one at least is universal;" there is an unconscious syllogism. A syllogism is then nothing artificial, but the natural process of unconscious as well as conscious thought. This illusion is one of the best examples perhaps of a universal and inseparable association, one which is no longer a process but has become a single act.

Ueber die scheinbare Grösse Gegenstände und ihre Beziehung zur Grösse der Netzhautbilder. GÖTZ MARTIUS. Philos. Studien, Bd. V, H. 4, S. 601.

In general, objects having large visual angles appear large and those having small visual angles appear small. But this general principle is materially limited in its application in the case of objects at different distances by the experience of the subject; the apparent size of objects does not decrease nearly as rapidly as their visual angles do. The experiments of Dr. Martius were to determine the accuracy of this sensory judgment or perception by inference. He hung up verti-

cally before a screen standard rods of 20, 50 and 100 cm. long and before a similar screen at a greater distance, other similar rods in succession varying slightly in length from the standard. The subject was required to choose that which seemed nearest the standard. His results are summed up in the following table. The standard rod was at a constant distance of 50 cm. from the eye of the subject; the comparison rod was in one series at 2.50 m. (left hand table), and in the other at 5.25 m. (right hand table) behind the standard; the figures are the excess in cm. of the rods which seemed equal to the standard rods over the length of the latter.

	Distance 50 cm. + 2.50 m.			Distance 50 cm. + 5.25 m.		
Length of standard in cm.	20	50	100	20	50	100
Observer M.M.	0.62	3.87	7.75	1.67	7.62	6.62
" G.M.	1.62	6.62	9.25	1.92	9.00	10.00

The table indicates according to the author that "the comparison-magnitude which seems at different distances equal to a given [standard]-magnitude increases constantly with the distance, but very slowly;" that "the absolute difference of the comparison-magnitude, which seems at a given distance equal to the standard-magnitude, increases with the latter;" and that "it is probable that the relative difference remains nearly constant [for all] at the same distance." For the further discussion of these results, which tell, as far as they go, in favor of the empirical theory, the reader is referred to the original.

III.—HYPNOTISM.

Der Hypnotismus, seine Bedeutung und seine Handhabung. A. FOREL. Stuttgart, F. Enke, 1889. pp. 88.

A part of this pamphlet originally appeared in an article in the *Zeitschrift f. d. ges. Strafrechtswissenschaft* (reviewed, AMER. JOUR. PSY. II. 316), and is now published with additions to meet the desire for a brief account of the more important facts of hypnotism. The scientific standing of the writer and his experimental knowledge of his subject make his work one to be most highly recommended. Among the additions is a section on the subjective aspect of hypnotism, in which are reprinted from the *Münchener Med. Woche ucrift*, Dr. Bleuler's experience as a subject, (noted below) and the author's own experience in auto-hypnotization (reviewed, AMER. JOUR. PSY. II.. 509).

Zur Psychologie der Hypnose. DR. E. BLEULER, of Rheinau. Münchener Med. Wochenschrift, No. 5, 1889.

The self-observation of an intelligent subject always has value and even more by reason of its rarity in hypotism. Dr. Bleuler entered the experiment with full will to be hypnotized, but endeavored to withhold himself from suggestion to learn its power. He thus describes his sensations on the first establishment of hypnosis. "My condition was now that of a pleasant and grateful repose; it came over me that I had no need at all to change my position, which under other circumstances would not have been continuously quite

comfortable. Mentally I was completely clear, observing myself; my hypnotizer could confirm everything objective that I afterwards told. By the suggestions that followed, the content of my conscious thought was not otherwise influenced than in waking; nevertheless they were in great part realised." On being told that he could not straighten his flexed arm, he felt his biceps tighten, entirely against his will, and counteract the extensors. On other occasions this was not felt; his will seemed then to have lost control over his muscles, or even to be itself paralyzed. He was able to reflect critically that the suggestion of an anæsthesia was made too early in the experiment, but felt pricks only as touches with a blunt edge. After repeated suggestion that he should wake next morning at a fixed hour, he passed a restless night waiting for the time to come; when the suggestion was otherwise given his sleep was undisturbed. Though he could long contend against a suggested action he was generally obliged to yield. Each new one, even if it was the cessation of action, seemed for the instant unpleasant. Suggestions of a complicated action could easily be resisted as wholes, but not when the successive acts necessary to its accomplishments were given separately. A post-hypnotic suggestion was with difficulty kept from fulfilment till wiped out by a night's sleep. Once an hallucination of taste was produced. When amnesia was suggested, but not very vigorously, there was difficulty in recollecting what happened, (once a brief period was wholly lost), and the subject could not fix the order of events. He was roused against his will by suggestion in about 10 seconds and without unpleasant symptoms.

Suggestive Therapeutics. A treatise on the nature and uses of hypnetism. H. BERNHEIM. Translated by Christian A. Herter. New York, G. P. Putnam's Sons, 1889, 8vo., pp. 420.

Of the French original of this work we said in May, 1888, "we regard [it] as on the whole the most scientific of the many works that have appeared in France within the present decade upon this subject, and we deem it a matter of serious regret that writers representing this method and stand-point were not chosen by the publishers of the International Scientific Series to present the subject to English and American readers, in preference to such thorough-going partisans of the school of Charcot, which ..... has been latterly so reluctant to accept the far better methods and results of Nancy." The regret then expressed has been met by the translation before us. Dr. Herter has had opportunity for observing the phenomena of which his author writes, with Prof. Forel of Zürich, himself a prominent representative of the Nancy school, and also in Paris at the Salpêtrière. His translation is readable, and the American public is to be congratulated on having the better side of the Paris-Nancy debate so well represented.

Hypnotism: its history and present development. F. BJÖRNSTRÖM, Translation from the second Swedish edition by Nils Posse. The Humboldt Library No. 113. pp. 124, 8vo. New York, 1889.

As long as comparatively few of the many foreign works on hypnotism are rendered into English and as still fewer are written in English, it becomes a matter of importance which books are chosen for translation, when as in this case the work is distinctly addressed to a large public and is published in a very accessible form. The

work of Dr. Björnström is a clear and orderly exposition of the chief points in modern hypnotism and is in many ways the most recommendable book for initiation into the field we yet have in English. Its history, the methods of inducing it, its stages, its physical and psychical effects, the importance of the term "suggestion," the applications of hypnotism in disease, education and law, its abuses and dangers are all included in small compass, precluding any detailed exposition, but fully enough for the general reader. The weak point of the volume is its lack of distinction between facts and the interpretations of them. The question of unilateral hypnotism and especially that of "mental suggestion" occupy too much space, and the conclusion in favor of these remote phenomena is accepted much too readily. This as well as the account of Parisian pseudo-hypnotism give the volume an unequal and even semi-sensational turn.

J. J

Der Hypnotismus. A. DERGAUC. Laibacher Zeitung. Mai, 1889.

A series of popular articles giving clearly though not always critically a survey of modern facts and methods in Hypnotism together with some account of its therapeutic, educational and forensic aspects.

Der Hypnotismus, seine Stellung zum Aberglauben und zur Wissenschaft. EUGEN DREHER. Berlin, 1889. pp. 33.

After a needlessly prolix discussion upon the distinctions between science and superstition, Dr. Dreher proceeds to give an account of the chief facts of hypnotism, and repeats the well-known story of the rescue of the topic from fields of charlatanry. One of the most positive points in the work is the denouncement of all telepathic hypotheses as falling into the realm of the superstitious, and as entirely opposed to scientific notions. The pamphlet makes very interesting reading, but is not particularly important.

A Clinical and Forensic Study of Trance. EDW. P. THWING. Medico-Legal Journal, June, 1889.

That the trance state may give rise to complicated legal questions, goes without saying. In this article the state itself and a number of these questions are taken up in a sketchy way by Prof. Thwing, with a view to directing attention to them. The great question to which these are mostly corollaries, is the immensely intricate one of "the relation of automatism to responsibility."

Hypnotism. G. T. W. PATRICK, Ph. D. North-western Journal of Homœopathy, May, 1889.

Prof. Patrick goes over the chief phenomena of hypnotism in an eminently sane and scientific spirit. Current information, even as it reaches the general medical reader, is apt to be more or less unreliable, and it is a matter of congratulation when those who are neither unbalanced enthusiasts nor bigoted conservatives engage in the work of popularizing it.

Leçons sur le grand et le petit hypnotisme. GRASSET. Reported by G. Rauzier, Revue de l'hypnotisme, Mai et Juin, 1889.

These three lectures set forth the characteristics of the Paris and Nancy schools of hypnotism, and Grasset's harmonizing of their opposing views. The chief argument of the Paris school (that of *le grand hypnotisme*) against the school of Nancy (that of *le petit hypnotisme*), is that they have no sure way of detecting simulation. The chief argument of the latter against the former is that all their characteristic phenomena are the result of suggestion. Grasset denies both these arguments; there are guarantees against simulation besides those used in Paris, and there are physiological effects besides those produced by suggestion. There are, indeed two forms of hypnotism, or rather the one neurosis (for Grasset regards the whole thing as morbid) has two groups of symptoms; one (*le grand hypnotisme*) is found only in connection with hysteria, and not always even there. The just claim of the Paris school to its title must rest on its having secured scientific attention for the outlawed phenomenon. In point of number of cases, of having caught the central point of the thing (suggestibility), and of therapeutic application, the Nancy school deserves the name of "the great hypnotism."

The Study of Hypnotism in France. JOSEPH JASTROW. Christian Union, Sept. 26, 1889.

The author outlines with characteristic clearness and interest the history and present status of knowledge in regard to hypnotism, distinguishes the views of the investigators of Paris and Nancy, describes the post-hypnotic phenomena and positive and negative hallucination, and points out the possible usefulness of hypnotism as a remedial agent together with the questions of criminal responsibility to which it may give rise.

Solution du problem de la suggestion hypnotique. AMÉDÉE H. SIMONIN. E. Dentu, Paris, 1889. pp. 129.

This little book sets forth what the author believes to be the solution of the problem of hypnotic suggestion. It also sets forth most forcibly his total ignorance of the way to a real solution of the problem and of the fundamental physiological conceptions necessary for it, both of which would at once appear, were it worth while to make citations.

Ueber psychische Beobachtungen bei Naturvölkern. ADOLF BASTIAN. *Die Magiker Indiens.* FRIEDERICH VON HELLWALD.

Both from the Shriften der Gesellschaft für Experimentalpsychologie zu Berlin. III Stück. Leipzig, 1890. pp. 32.

The reward for pushing one's way through the many twistings and turnings thickly strewn with obstructing parentheses and scraps of polyglot illustration, that characterize the writings of this suggestive but obscure anthropologist, is in the present case a very interesting though arbitrarily eclectic survey of that field of mental action, that is common ground to science and superstition. On the one hand we find the same pseudo-scientific pretenses that succeed with the unlearned even amongst the elite, in the customs and thought-habits of savage people; on the other hand we find the

scientific portions of this field, the various methods of producing hypnotism, etc., capable of explaining the more wonderful portions of savage doings and conceptions. Many pertinent illustrations of these facts are given and they lead the author to conclude that it is precisely in primitive peoples that we find as more or less normal most of those phenomena producible in us only artificially and not without danger to the subject; so that the simple observation of well prepared travellers would lead to an unsuspected extension of the field of research."

Quite in the same strain von Hellwald cites cases in which magic, self-induced hypnotism and other factors enter into the complex operations of the fakirs and other oriental priest classes. J. J.

Des hallucinations négatives suggérées. BERNHEIM. Revue de l'hypnotisme, Fév., 1889. Réponse à M. le Professeur Delbœuf.

In commenting on a paper of Bernheim's in the January number of the *Revue*, Delbœuf insisted on the very great and intelligent part played by the subject in case of negative hallucination, in language which might be construed to mean that the whole thing was simulation (*vide* AMER. JOUR. PSY. II, 324). To prevent such a misapprehension, Bernheim again defines his position, asserting the complete freedom of genuine cases from all simulation. Unilateral blindness in hysteria is equally real and equally psychic, and occurs where the ignorance of the subject, both of the defect and its connection with her disease, guarantees the impossibility of simulation. With the hypnotic subject the sensations reach the cortical centers, but fail of the further processes, whatever they may be, needed to bring them to consciousness; they are, so to speak, unconsciously perceived. With hysterics these further processes are not lacking, but "the imagination of the subject without her knowledge neutralizes" the sensation.

Quelques remarques sur suggestion. AUGUST FOREL. Revue de l'hypnotisme, Avril, 1889.

In this somewhat rambling paper, Prof. Forel touches upon a number of interesting points in regard to suggestion and auto-suggestion (which are the same as far as the subject is concerned), the process by which the operator secures control of the mental machinery of the subject, the unpleasant after-effects of hypnotization (due to auto-suggestion and to be suggested away by the operator), means of making subjects auto-hypnotizable, etc., etc. Apropos of the discussion of Bernheim and Delbœuf, he mentions the arrest and recall of the senses as unsimulable by the subject. In his opinion, "it is not only the will of the subject which is sometimes more, sometimes less completely directed by the hypnotizer, but his whole cerebral dynamism, sensorial (centripetal), motor (centrifugal), and intellectual (central) alike."

Les perceptions inconscientes de l'hypnotisme A. BINET. Revue Scientifique, Fév. 23, 1889.

Binet complains of the misconceptions arising from confusion of terms in hypnology, citing as an example the recent papers of Liégeois, Bernheim and Delbœuf on negative hallucination, which

were supposed to refute the views of his party, whereas they were actually in agreement. He now defines that phenomenon, and enumerates the facts agreed upon. As a name for the phenomenon, Binet himself with Féré, proposed "systematic anæsthesia." To avoid confusion, however, he is willing to accept "unconscious perception" or "negative perception," terms certainly more fitting, in view of recent experiments, than "negative hallucination."

De l'amnésie rétroactive dans le sommeil provoqué. BERNHEIM. Revue de l'hypnotisme, Juillet, 1889.

Certain subjects on waking from the hypnotic trance, are without memory, not only of what has happened during that state, but of events immediately preceding it, and this may happen spontaneously with persons that have never before been hypnotized nor seen others hypnotized. A case of this kind is described by Bernheim. No explanation is offered, but the similarity to retroactive amnesia in fevers and alcoholic delirium is recalled.

Note sur l'enregistrement des excitations portées sur une région anesthésique du corps chez les hystériques. ALFRED BINET. Compt. rend. de la Soc. de Biologie, séance du 12 Jan., 1889.

The motor effects now reported are in the line of the curious optical effects secured by the same experimenter by the stimulation of anæsthesic areas on the bodies of hysterical patients (see abstract, AMER. JOUR. PSY. II., 324), and are equally suggestive with reference to the conscious unconsciousness of some hypnotic subjects. He finds that stimulation of an anæsthesic area is followed by a slight, unconscious muscular excitation, the effect of which is to be seen in myographic tracings taken on the muscle masses of the limbs, trunk and face. Stimulation of a sensitive area produces a less widely irradiated excitation in many subjects than stimulation of an anæsthesic area, and in other particulars also there are variations of result. The rhythm of stimulations is sometimes followed, and the duration and intensity of the stimulus is of effect on the trace, which make it seem probable that, in spite of the subject's unconsciousness, psycho-motor centers take part in the response.

Recherches sur les altérations de la conscience chez les hystériques. A. BINET. Revue Philosophique, Fév., 1889.

It is well known that the anæsthesias of hysteria are vastly different from those of organic lesions, and the researches of M. Binet make this yet more apparent. The tests were made out of sight of the patient, without previous suggestion or hypnotization, and his results are, he thinks, symptomatic of the disease. It appears that, while simple pricks and touches produce no reaction, stimuli with some meaning (a dynamometer placed in the hand, or a pencil in the position for writing), may provoke a characteristic response of adapted motion. In some subjects, the presence of all the kinæsthetic senses and that of pain can be demonstrated. Furthermore, this action is not independent of consciousness. The thoughts of the subject find expression (in the case of automatic writers) on the anæsthesic side, and the sensations of that side reach consciousness though not with their normal character, tactual sensations generally

calling up visual ideas. Binet is inclined to suspend judgement on the question of the conscious perception of these sensations by a second "personality," but argues from his experiments that this doubling of "personality" cannot arise in dissociation or splitting of the ideas into two independent groups, as some have contended. The elaboration of the responses and their connection with consciousness support the cortical theory of hysteria long taught by Charcot. Regretable as it is that such important experiments must be conducted upon such uncertain subjects as hysterical patients, and that M. Binet is not over-skeptical in regard to the action of the magnet, his experiments do not fail of a very great interest.

Ueber Hypnotismus und Suggestion, sowie deren therapeutische Anwendung in der ärztlischen Praxis. FRANZ MÜLLER. Wien, 1889. Moritz Perles. pp. 20.

Believing that there is still a large number of physicians who are skeptical regarding the applications of hypnotism in medicine, Dr. Müller prepares for them an excellent common-sense statement of what is meant by suggestion and how it acts. He takes forcibly and throughout the position of the Nancy school and insists that the process is psychical in every phase. After citing the usual cases of the influence of mind over body, he reminds us that hypnotism is not a panacea, as some claim; it has its distinct limits, largely the same as the limits within which such factors as a cheerful mien, impressive and hopeful manner are effective. "The domain of the therapeutics of suggestion is occupied primarily by the hysterical complex of symptoms; secondly, paralyses, cramps, and neuralgias caused by psychic shock; thirdly, neurasthenic troubles and sleeplessness, and finally pains and neuralgias." A point noted in combatting the purely physical theory of hypnotization may be cited. Dr. Müller asks why, if as many claim a bright light produces hypnosis, do not ophthalmologists find amongst their numerous patients cases of spontaneous hypnotism when the ray of light from the ophthalmoscope is thrown upon the retina? The query is certainly in point, and argues that the light is efficient when its effect is expected. J. J.

Ueber den Hypnotismus und seine Verwerthung in der Praxis. Dr. W. BRUGELMAN. Berlin, 1889. pp. 20.

This pamphlet is interesting as an additional endorsement from German soil of the therapeutic application of hypnotism. The position taken by Dr. Brugelman presents nothing peculiar. He regards hypnosis as closely affiliated with normal sleep, and as an important instrument in the hands of a competent physician for alleviating not all but an essential portion of human ills. The pamphlet is deficient in not recognizing the characteristic distinctions between the schools of Nancy and Paris, but in his own practical advice and treatment of cases he follows the former. In discussing the causes of the variations in the percentages of hypnotisable persons Dr. Brugelman makes the apt suggestion that the local environment is a most potent factor; in Nancy, for example, there has grown up a generation accustomed to being hypnotised and to regarding it as an every-day process. Hence the percentage is high.

Die Suggestionstherapie und ihre Technik. EDWARD BAIERLACHER. Stuttgart, 1889. pp. 59.

This is a purely practical treatise, dealing with the chief facts, methods and uses of the hypnotic process. The essay falls into two parts, the first on the process and the second giving a list of cases with appropriate criticisms. The former gives a convenient résumé of the chief points necessary for the practitioner, the latter shows the large variety of diseases to which the treatment is applicable. The success attending the treatment elsewhere will doubtless soon lead to a speedy extension of it throughout Germany.

Clinique de Psycho-Thérapie suggestive fondée à Amsterdam par les docteurs A. W. VAN RENTERGHEM ET F. VAN EEDEN. Bruxelles, 1889. pp. 92.

This comprehensive report read at the recent Congress of Hypnotism in Paris, places in convenient form the results of the "suggestive" treatment of 414 cases covering a period of two years. These two physicians took as their model the clinic of Liébeault in Nancy, and proceed entirely on the methods of the Nancy school. They have a suite of five rooms in a hotel in Amsterdam, comfortably arranged with easy chairs for the patients, with precautions against ill effects, and provision for pleasant impressions upon awakening. Their methods are strictly scientific, and they are actuated by a desire of demonstrating the power of suggestive-therapeutics when scientifically applied. They confine their efforts simply to the cure of the patient, suggesting repeatedly that the pains will vanish, that functional irregularities will disappear, that the power over a lost limb or a lost sense will return, that sleeplessness or mobid appetites will no longer trouble the patient, and so on. Each case must be treated on its own merits and according to the character of the individual. The suggestion must, where possible, be aided by initiating in the hypnotic state the effects desired after awakening. Regarding the results they tabulate all their cases, appending histories in typical instances, and subdividing them into the following ten divisions, of which the first five are affections of the nervous system, and the others of parts other than nervous. The number of cases treated under each division is added. 1. Organic (?) affections, 29. 2. Serious neuroses, hysteria, 40. 3. Mental diseases, 60. 4. Neuropathic troubles, 164. 5. Various nervous pains, 68. 6. Internal functional troubles, 27. 7. External functional troubles, 17. 8. Fevers, 1. 9. Menstrual troubles, 7. 10. Anæsthesia for surgical purposes, 1. Taking first the statistics in general we find 53 per cent. men and 47 per cent. women; we find about 4 per cent. remaining uninfluenced by the suggestions, 56 per cent. falling into a light sleep, 32 per cent. into a deep sleep, and 11 per cent. reaching the stage of somnambulism. Of the 414 patients 9 were between 1 and 10 years of age, 46 between 11 and 20, 203 between 21 and 40, 131 between 41 and 60, and 25 between 61 and 80. On 20 per cent. the treatment had no beneficial effect; on 26 per cent. it produced a slight or passing amelioration; on 27 per cent. it produced a marked or permanent amelioration; and 28 per cent. were cured. Regarding next the nature of the disease the vast predominance of nervous troubles at once suggests the special field of hypnotic therapeutics; of non-nervous disease classes 6 and 8 have the greatest chances of cure by this treatment. Of 29 organic affections we find but a single case of cure, (and even that in doubt), showing the

absurdity of the panacean claims sometimes advanced. The more serious neuroses are almost all decidedly improved, but only 9 in 40 completely cured; mental diseases show a small percentage of cure and a large percentage of negative or slight results. Class 4 shows an excellent record, 39 slightly, 37 markedly improved and 47 cured, while class 5 shows 11, 19 and 17 in the same divisions. The tables contain many more interesting facts, and will in the future be constantly referred to as amongst the first sufficient data for a reliable conclusion regarding suggestive therapeutics. This conclusion must declare the method in comparison with other methods an excellent success, particularly with functional troubles of the nervous system, slight and serious. J. J.

Das Doppel Ich. MAX DESSOIR. Berlin, 1889, 8vo. pp. 42.

In this very entertaining lecture of a prominent member of the Berlin Society for Experimental Psychology, we have a consistent elaboration of the theory that has played quite a rôle in the history of Psychology, that the mind is dual in nature, that there are two of each of us. But this *alter ego* is not the tenant of the other half of the brain as Wigan had it; it is rather a secondary form of consciousness, an under consciousness which in ordinary cases occupies the humble quarters assigned to it but occasionally makes itself prominent and exhibits curious and bizarre forms. When we are deeply engrossed in work and yet something within us counts the strokes of the clock unknown to the worker; when we practice ourselves in this art so that we can keep up a lively conversation and at the same time perform intricate additions and multiplications, then both our egos are working at once. Ordinarily the upper consciousness alone does the work of direction, but occasionally the under consciousness takes the reins. When we automatically do things that are habitual and take no cognisance of it, as when we abstractedly take the night-key with us on going out and then suddenly start to go back for it, unaware that the under-consciousness has already attended to it, we are in the hands of the "*Doppel Ich*." So much for the normal. This germ of a second personality which we each carry with us may develop unusual forms until it passes step by step into the pathological. When in dreams we take up the thread one night where we left it the night before, when the drunkard remembers in one intoxication what he did in the previous one, (though in the sober condition unable to do so), these changes of bodily condition seem the sufficient starting points for a disintegration of the self. From this the steps are gradual to the seizures of the epileptic, to those cases of sudden amnesia in which every faculty seems to be lost and then as suddenly regained, and finally to such complete instances of dual personality, as Dr. Azam's case of Felida X, whose normal conditions form a complete chain by themselves, but the links of which are alternately interrupted by the secondary states equally continuous amongst themselves, but no remembrance of the normal condition remaining in the secondary, nor vice versa.

But the subject has an experimental side as well. Hysteria and hypnotism are the fertile fields in which such experiments flourish. In one hypnotic trance patients remember what they did in former ones though they forget it in the intervals; in the execution of post-hypnotic suggestions we see all shades of relation between the hyp-

notic and the waking *egos*, some patients falling into a sort of hypnosis again, others performing the act unconsciously, and still others doing it consciously and offering a lame excuse for it. Still further, a few cases have been described, notably one by M. Pierre Janet, in which the hypnotic personality regularly assumes a constant personality but one differing from the normal and entering into the most complicated relations with it. Indeed a third personality emerges by the hypnotization of the abnormal personality. The proposition which Dr. Dessoir reaches and in which M. Janet and Mr. Myers concur is that the hypnotic state consists in "an artificially induced predominance of the secondary *ego*." To prove this a large number of the experiments, some very ingenious and others very inconclusive, are undertaken to appeal indirectly to the ordinary consciousness, which in the hypnotic state is the subordinate one, and gain the evidence of the two personalities existing side by side but with the usual relations reversed.

The point of view thus taken is certainly an interesting one, but is it not expressing, with an undue emphasis upon that unknown factor of personality, the current doctrine that in hypnotism we have an automatic state, a loss of voluntary control and an exaggerated suggestibility in all directions? The "double-*ego*" is a convenient phrase for bringing into connection various groups of facts, but in its extreme form it loses its utility, and as a theory of hypnotism it is neither so lovel nor so important as its upholders believe. J. J.

Mary Reynolds: A Case of Double Consciousness. S. WEIR MITCHELL. M. D. Reprint from the Transactions of the College of Physicians of Philadelphia, April 4, 1888.

Mary Reynolds was born in 1793 and died in 1853; her case is therefore not of recent observation, and Dr. Mitchell is compelled to depend upon testimony. The contribution is nevertheless a very acceptable one. At about eighteen the girl began to have hysterical "*fits*." A little later, in a period of prolonged sleep, she experienced her first change of consciousness, and thereafter for fifteen or sixteen years continued to pass at irregular intervals from one state to the other, being left at the age of 36 in the second stage. In the first she was retiring and melancholy; in the second, fond of society and light-minded. When however she finally rested in the latter state the mental disturbances sometimes attending it gradually disappeared; she became by degrees more sober also, but without losing the prevailing color of the state. Specimens of her script in the two states by their remarkable similarity point a moral upon the treacherousness of popular testimony as to changes of handwriting in such cases. The numerous particulars however which multiply the interest of the original do not lend themselves to summary here.

Des hallucinations suggérées a l'état de veille. E. YUNG. Revue de l'hypnotisme, Mars et Avril, 1889.

Prof. Yung has made a large number of experiments in the suggestion of mild and transient hallucinations (*i. e.* hypnotic suggestions in embryo) to normal people—not in special and unusual circumstances, but in those of every-day life. The experiments succeeded better, but by no means exclusively, with women and children and the uneducated. They require a certain state of mind

in the subject, which it is the business of the operator to produce by authoritative manner, earnest assertion, and in general by the arts of the "magnetizer." The author has some 30 drawings made by students in his microscopical laboratory of objects which they saw and drew from blank slides after authoritative description by him of what they were to find there. By means of the parlor games of the 'magnetized card" and the "scented coin," both fully described in the paper, Prof. Yung secured a large number of observations. Of 600 experiments made with the first, only 83 (13.8 per cent.) failed completely: 68 on men, 15 on women. The remaining 517 fall into 4 groups: 212 recognized the "magnetized card" according to instructions, by a shock in the muscles of the fingers, the hand, the arm, etc.; 95 to whom only a general touch sensation was suggested had various tactile sensations; 52 saw the card move on assurance that it was so to be recognized; and 158 knew it by an odor. Those that have been hypnotized are, as might be supposed, most susceptible to these hallucinations, but Yung believes that everyone sufficiently prepared, as described above, may become a subject.

IV.—MISCELLANEOUS.

Elemente der Psychophysik. GUSTAV THEODOR FECHNER. Zweite unveränderte Auflage. Leipzig, 1889. Breitkopf und Härtel. 2 vols., pp. 336 and 572. 16 mks.

This second edition of Fechner's epoch-making work follows closely upon the death of its author. The work has long been out of print, and copies advertised for sale were always eagerly sought. Fechner himself did not care to undertake the necessary revision for a second edition, nor did he think it right to re-issue the first just as it stood. Prof. Wundt is the editor of the present edition, which differs in no way from the first, except in a few verbal changes and the incorporation of notes referring to Fechner's later works. There is also appended a very complete bibliography of Fechner's work, mainly by Dr. Rudolph Müller. When we remember that in this work the problems of psychophysics find their first systematic statement and elaboration, that many of the observations therein described still remain as the basis of current views, and that the historical value of the book must continually increase, we feel the necessity and propriety of the re-issue of this treatise, and offer our thanks to both editor and publisher for again placing the most important outcome of a talented savant's life in the reach of all students of psychophysics. J. J.

Handbook of Psychology: Senses and Intellect. JAMES MARK BALDWIN, Ph. D. New York, Henry Holt & Co. 1889. pp. 325, 8vo.

In the present aspect of Psychology a restatement of its problems and results in a convenient and readable form would be welcomed by a large company of students, who on one hand are repulsed from the host of individual systems by their unprovable statements, and on the other are equally deterred by the scattered results of the new psychology. To these the present volume will be a disappointment. It is difficult indeed to find its *ratson d'être;* there are abundant equally meritorious text-books without adding to their number. As

representing the distinctive field of modern psychology, which in part it aims to do, it is hopelessly deficient in the statement and arrangement of fact, as in the presentation of general views and theories. J. J.

Know Thyself; or Psychology for the People. A. W. HOLMES-FORBES. Dublin, 1889, pp. 52.

The aim of this pamphlet is to present some of the more general laws of mind in readily comprehensible form. The author is a dualist and finds in the mind by introspection three parallel orders of phenomena, one arising from the immaterial soul alone, one from the body alone, and one from their union. It is hardly necessary to say that his psychology is neither physiological nor experimental.

Sinneswahrnehmung und Sinnestäuschung. Dr. E. REHFISCH. Berlin, 1889. pp. 62.

The author's intention is to present for the benefit of the "laity" a view of the processes of sensation and sense-illusion in the light of recent physiological progress; and in this he succeeds most admirably. The point of view is typically that of the new Psychology, and the entire presentation most satisfactory. The first half deals with the physiological substrata of sensation, accentuating the different factors of the process in relation to the various centres of the brain; in so doing Dr. Rehfisch does not confine himself to a matter-of-fact series of statements, but surrounds the expositien with many interesting and pointed illustrations. The second portion of the treatise takes up the morbid side of sensation, making the usual distinctions and enforcing them with cases in point. There is nothing original about the werk, but it is simply what it pretends to be,—an easily comprehensible general review of the problems of sensation. J. J.

NOTES.

An interesting illusion affecting the sense of resistance is noticed by Goldscheider in the *Centralblatt für Physiologie*, No. 5, 1889. When a weight is held suspended by a cord and the hand is carefully lowered till the weight rests upon some support and relieves the hand of its downward tension, there is a sensation of resistance to further downward motion; and the resistance is located at the place of the weight. When the weight is sufficiently heavy, the illusion may equal the sensation of touching the support with a rod. The explanation is evidently in the persistence of the muscular tension originally required to support the weight. There is no upward pressure on the skin nor stimulation of any other superficial organs; the illusion and the perception of resistance rest upon some deep-seated sensation, probably that in the joints. The same sensation is present of course in touching with a rod or with the finger alone, though the dermal sensations are added; it is therefore in part at least in the joints that this important factor of belief in the reality of the external world is located. The projection of the sensations of resistance to the end of the rod or to the weight depends on the presence of other ideas which give a preconception of that location.

Thomas W. Engelmann, the discoverer of the photometric bacterium, has continued his observations upon another group of these light-responsive organisms, (*Botan. Zeitung*, No. 42-45, 1888, cf. also *Pflüger's Archiv.*, XLII, p. 183). These are mostly "sulphur bacteria" and all contain a diffuse red pigment (bacteriopurpurin), from which he calls the group *Purpurbacterien*. Their behavior toward light is something as follows: When the illumination is not too long continued, their activity increases with the intensity of the light. Long illumination, as complete darkness, brings them to rest. Sudden decrease of intensity causes a sudden backward movement (*Schreckbewegung*) of some time ten or twenty times their length, followed, if the light continues at a lower intensity, by slower forward movements. Sudden increase of intensity causes more rapid forward movements if the rate is not already very great. In a micro-spectrum they gather thickest in the ultra-red, next in the orange-yellow, and less toward the violet, their preference corresponding with the place of greatest absorption by the bacteriopurpurin. The effect of light on their movements Engelmann believes to be directly proportional to this absorption. Like chlorophyll, the pigment sets free oxygen in the light, a fact neatly demonstrated by the thronging of bacteria sensitive to oxygen about the *Purpurbacterien*.

M. J. Fontan, like Bernheim and others, has made the observation that in the cases of negative hallucinations in hypnotism, the physiological processes are carried out though the psychic ones are inhibited. When a subject acts out the suggestion that he sees nothing red, his pupil none the less contracts when a bright red is shown

and dilates when exposed to a dark red. Better still; let the suggestion be given that red is invisible but when a bell is sounded the vision for red will return. Now let the subject gaze fixedly at a red light and as you sound the bell put out the light, and the subject declares he sees a green light. In other words the retina and the lower centers have been impressed and have performed their function of bringing out the complementary color, though the subject's mind declares itself utterly unconscious of the impression. M. Fontan also makes other apt suggestions on the nature of negative hallucinations or unconcious perceptions as he prefers to term them. (*Revue Scientifique*, May 11, 1889.)

In a preliminary communication in regard to the heat regulation of the human body, Loewy (*Pflüger's Archiv.* XLVI, p. 625) states that he has obtained the following results The regulation is not perfect; the temperature of the body falls, without exception, upon the withdrawal of a large amount of heat, and in most individuals upon the withdrawal of a small amount. The skin is the principal organ for the regulation of the temperature; cold causes a reflex contraction of the skin and of its vessels, and hence a diminution of the heat dissipation. The heat-production remains unchanged (and hence plays no part in the regulation), so long as no muscular contractions take place. Muscular contractions can be inhibited, even when there is a very considerable fall in the temperature of the body. Extreme and lasting cold produces involuntary contractions. The increased decomposition produced by these contractions (trembling, etc.) is very considerable. It may reach 100 per cent. and probably much more. But still it is not sufficient to prevent a further fall of temperature. This means of regulation is not of practical importance, because we seek artificial sources of warmth long before it is called into play. It does not appear that Loewy made any investigation into the changes in the activity of the sweat-glands, as acted upon directly by their nerve-centers, and not indirectly by their blood-supply.

W. A. Turner, in a communication on the innervation of the muscles of the soft palate (*Jour. Anat. Phys.*, XXIII., 1889, p. 523), concludes: 1s, that there is not sufficient experimental or clinical evidence to support the doctrine that the muscles of the soft palate are supplied by the portio dura; 2nd, that experimental evidence shows that these muscles are innervated by the internal branch of the spinal accessory (*nervus accessorius vagi*), whose fibres are distributed along with certain branches of the vagus; 3d, that sufficient clinical evidence exists to prove that paralysis of the palate results from disease affecting the medullary center, the roots and the peripheral distribution of the vagus and its accessory nerve; 4th, that with the paralysis of the palate (*palatoplegia*) there is associated paralysis of the tongue (*glossoplegia*) and of the vocal cords (*laryngoplegia*), either unilateral or bilateral, according to the situation of the disease. F. T.

The profound effects following the emotional strain of great calamities has been frequently remarked. Those of the late Johnstown disaster are thus described. "Most of the faces that one meets, both male and female, are those of the most profound melancholia, associated with an almost absolute disregard of the future. The

nervous system shows the strain it has borne by a tremulousness of the hand and the lip in man as well as in woman. This nervous state is further evidenced by a peculiar intonation of words, the persons speaking mechanically, while the voices of many rough-looking men are changed into such tremulous notes of so high a pitch as to make one imagine that a child on the verge of tears is speaking. Crying is so rare that your correspondent saw not a tear on any face in Johnstown, but the women that are left are haggard, with pinched features, and heavy, dark lines under their eyes. Indeed the evidence of systemic disturbance is so marked in almost every individual who was present at the time of the catastrophe that it is possible with the eye alone to separate the residents from those outside."

An interesting point of connection between passion and epilepsy (the resemblance of which was remarked by Echeverria) is noticed by Dr. Ch. Féré in the *Revue de l'Hypnotisme* for June, 1889. Observations on blood pressure during the aura show a rise of 200–300 gms. This high pressure continues through the convulsions, but gives place after the attack to one below the normal, which lasts 8–10 hours or longer, and in serial attacks even for several days. A rise of the same sort occurs in the attacks of mental excitement and agitation. Here cupping or mustard baths reduce it very materially (with a concurrent decrease of the symptoms), though the reduction which they effect is slight upon subjects in the normal condition. It is known that violent effort or emotion (both attended by increased pressure) may bring on the fits. One of Féré's cases, an epileptic with a normal pressure of 800 gms., showed 1050 in a period of attacks and 1100 in a fit of anger. An angry imbecile had a pressure of 1000–1100 against 850 when undisturbed; an angry cabman 1100 against 800.

In the *Medical and Surgical Reporter*, Feb. 9, 1889, Dr. Geo. M. Gould gives three cases, one of chorea, one of flatulent dyspepsia of twenty years standing, and one of palpitation of the heart, in which the trouble seems to have been reflexly caused by eye-strain, and ceased after the correction of the ocular errors. In the number of the same journal for March 9. he explains the alleged greater frequency with which eye-strain results in troubles of the eyes themselves before puberty, and in headaches and reflex neuroses after puberty, especially in women. This he does chiefly by reference to the general principle of sexual selection; reflex troubles are less of a hindrance in securing a mate than visible and disfiguring troubles of the eye would be, besides leaving an important organ free to perform its function. In the general causation of eye-strain, the near work required by present civilization is of course an important factor.

A case of epilepsy, described by Drs. Hughlings Jackson and Beevor before the London Medical Society (*Brit. Med. Jour.*, Feb. 23, 1889, p. 414), is of special interest, as going to support Ferrier's location of the olfactory center in the hippocampal lobule and adjacent *parts*. The fits were not severe, the patient neither fell nor lost consciousness, but as a part of her aura had an indescribably horrible smell. Post-mortem examination showed a tumor at the end of the tempero-sphenoidal lobe, surrounding the amygdaloid body and crowding upon the dentate nucleus and the fibers of the internal capsule; the cortex of the hippocampal convolution was uninjured.

The presence of an olfactory hallucination rather than anosmia shows the center to have been excited but not destroyed.

Prof. Venturi, whose study of temporary insanity was noticed in this JOURNAL (Vol. II, p. 178), has added a new case of temporary melancholia, (*Archivio italiano per le mal. nervose, etc.* 1888, XXV, 369). The patient was a temperate and industrious man, 32 years old, and previously of sound health and of sound heredity. After a not inexplicable fit of ill-humor (lasting from one day through the next, but not interfering with sleep or work), just after going to bed, he suddenly fell into the deepest melancholic agitation. On examination 3 hours later, he showed on the psychic side profound disturbance of consciousness, delirium of fear with visual illusions, mental distress, attempts at flight, though not at violence to himself or others, (there were thoughts of suicide at first), and on the physical side, dilated, but responsive pupils and cutaneous hyperæsthesia. Under morphine he quieted down, and after 3 hours' sleep awoke much improved. He slept again in the forenoon and by evening was given over as perfectly restored, though with incomplete recollection of the attack. The insanity did not outlast 14 hours. Dr. Sommer in abstracting the paper for the *Neurolog. Centralbl.* No. 8, 1889, remarks that while such cases rarely come under scientific observation, they are not really so rare as might be supposed. The city-asylums in a large city could undoubtedly furnish a good many examples.

The hold taken by scientific ideas of inebriety is shown by the character of the asylum just started by Prof. Forel of Zürich and Dr. Bleuler. There is a farm connected with the institution on which the patients labor. Total abstinence is enforced; labor is compulsory; the patients are received for a minimum period of six months; hypnotic suggestion is employed; and persons decidedly insane are not received.

The following questions for the discovery of those sensitive to "odic" force are taken from an account of the work of Reichenbach in the *Revue de l'hypnotisme*, Juin 1889. Most of them seem to be the generalizations of experience, and it is easy to see that one who could answer many of them in the affirmative would have the hypersensitive nervous organization and the general suggestibility that would make him a good subject for the demonstration of any occult phenomenon—"odic," or hypnotic.

1. Is your sleep calm or disturbed?—The sensitive generally has disturbed sleep.
2. Can you sleep two in a bed?
3. Do you not experience uneasiness when you find yourself in the midst of a crowd at the theatre or in a church?
4. When you give your right hand to any one is it indifferent to you that the pressure of the hand is prolonged?
5. When you handle pieces of money, or better of copper, is it indifferent whether you do so with your right hand or your left? Do you put the money in your right-hand pocket or in the left?
6. Do you experience any sensation when you look at yourself in a mirror? Is the sensation agreeable or disagreeable?
7. When you set the ends of your ten fingers lightly against a wall do you not after a while experience a sensation of heat in one hand, of cold in the other? Which of your hands is cold?

8. When you hold the palms of your hands one beside the other, and they are lightly blown upon is there a difference in the sensation felt by the two hands? Which of the hands has a sensation of heat, which experiences a sensation of cold?

9. Is it disagreeable to you to have flowers in your room at bedtime?

10. Passes made over your hand, your arm, your face, your body – are they felt?

11. Is the direction of these passes indifferent?

12. Do you like sweet and rich food? acid and salt? bitter and astringent?

13. Do you talk in your sleep?

14. Do you distinguish blue and yellow with reference to the impressions produced?

Finally, if it is desired to discover the degree of sensitiveness of the subject, it will be sufficient to make use of passes at a distance.

In a note in the *Revue de l'Hypnotisme* for June, 1889, Dr. E. Dupuy mentions a case of most unusual control over the ocular muscles. The person in question, a woman of 27, was able to move one eye in any of the ordinary planes of motion while she kept the other fixed. She used either eye at will for vision during these motions but not both, the attempt to do so giving rise to a sensation of falling. Her ordinary binocular vision appeared uninjured by these ocular gymnastics.

The following definition of a living being is given by Fernand Lataste in the *Comtes Rendus de la Soc. de Biologie, séance du 5 Jan.*, 1889: "A living being is a being composed of elements, in incessant chemical renewal and reacting upon one another in a way to maintain the form and functions [of the being] in a determined cycle of evolution, similar to the cycle traversed by other living beings from which the one under consideration comes forth or to which it is bound by community of origin."

PERISCOPE.

The remarkable awakening and concentration of interest upon psychological themes within the past few years indicates not only increased activity but a change of direction, if not even of base. More and better methods of both observation and experiment have yielded important results and promise far more important ones in the near future. The movement is peculiarly significant because it indicates that man is to attain a better and more objective knowledge of himself than ever before, and at the same time almost all valid results have immediate applicability in educational methods, mental hygiene, preventive medicine and moral and religious opinion.

It may interest our readers if we note a few important movements in the field represented by the AMERICAN JOURNAL OF PSYCHOLOGY within the two years of its existence completed by this issue.

Within this period falls the first international congress for criminal anthropology, held at Rome and organized by Lombroso, Ferri and others to discuss such questions as the following:—is there a general bio-pathological character predisposing to crime and can different sources and types of it be distinguished, is suicide in inverse rela-

tion to murder; moral insanity and epilepsy in prisons and insane asylums; classification of criminals; simulation and dissimulation. A large exposition of skulls, brains, photographs and graphic tables was opened in connection with this congress. While Italian psychology has made its most valuable contributions in this field much besides has been done. Sergi has digested and at several points amplified the work of the English school, making as the strange center of his system the idea of the pathological character of all religions. Golgi has made his remarkable contributions of new methods and results in the field of brain histology. Mosso, one of the most genial and talented of living physiologists, is devoting himself more and more to problems of a chiefly psychophysic character, while many younger men represent nearly all the modern tendencies to be found in Europe in the psychological field. Fresh modern research is taking the place of the speculative views, which far from holding their own in the rapid and new University development of that country, have steadily lost ground since the death of Vera.

In France the exposition has seen the formation of *L'association Internationale d'Hypnologie*, of which M. Bérillon was the leading spirit and the origin of the *Congrès internationale de psychologie expérimentale*, under a permanent committee of 26, and with a membership of nearly 400. The *Revue de l'Hypnotisme* has added to its name '*et de la psychologie physiologique*' and modified its contents accordingly. The school of suggestion has carried all before it in the study of hypnotism, (the therapeutic value of which is daily proven,) and has a new monthly organ *La Clinica*, edited by A. S. Herrero. In the *École pratique des Hautes Etudes*, Professor Beaunis has opened a new laboratory for physiological psychology in the building of the new Sorbonne. The *Société de psychologie physiologique* has now been established nearly two years, and began with thirty titular members residing in Paris and corresponding members in the provinces. Its work, so far as we can infer from its publications in the *Revue Philosophique*, is mainly upon phenomena which may be called abnormal, but not morbid, and promises to shed much light upon current popular delusions like spiritualism, telepathy and mind cure. Ribot has been made professor of physiological psychology, first in the Sorbonne and then in the College de France and has continued to make the most convenient and popular of all digests of the results obtained by German savants in both morbid and experimental psychology.

In England very little has been accomplished in empirical psychology, save the valuable anthropological work of Galton, and the work of Horsley and of Schäfer. *Mind* has more than held its own as a model of editorial care, and the Society for Psychical Research, without expressly relinquishing its telepathic theories, is gradually turning its energies into more fruitful and more promising channels.

In Germany hypnotism has made much and sure progress as a therapeutic agent, but almost solely on the basis of the theory of suggestion and chiefly among alienists, where observations are more painstaking than those made in France. The laboratory of Wundt has been much enlarged and now comprises half a dozen well filled rooms. Kraepelin at Dorpat, Preyer at Berlin and Krafft-Ebing at Vienna have begun labors in their new fields of work in lines represented by this JOURNAL. Münsterberg at Freiburg has commensed a very promising and comprehensive series of "contributions to experimental psychology," Ebbinghaus and Goldscheider at Ber-

lin, G. E. Müller, the successor of Lotze at Göttingen, Monakow and Forel, Moll, Loeb, Dessoir, Edinger, Benedict and scores of others have made valuable contributions from their various standpoints. Noteworthy is the growing tendency to psychological lines of investigation by physiologists. This is seen not only in Pflüger's and Du Bois-Reymond's Archivs, but in Holmgren's new Scandinavian Archiv, in the work of Setschenow's pupils in Russia, particularly Orschansky at Charkow and among American physiologists, particularly in the valuable work of Bowditch of Boston. In Germany the history of philosophy seems settling into a standard university course desirable for all who can give time to it. Kant philology and the theory of knowledge are nearly exhausted lines of work, but even in these courses it is noteworthy that psychological subjects enter at every possible point and everywhere give zest. Psychology has in Germany already awakened the hopes and fears of religious leaders who see in it a great and rising movement. Even in Scandinavian universities the all pervading influence of Boström yielding to the younger psychologists.

In this country Dr. Jastrow has established rooms for psychological research in the University of Wisconsin, where several determinations have been already made, which will be published in the next number of this JOURNAL. Dr. Cattell has a laboratory for psychological study in the University of Pennsylvania. Dr. Wolfe is introducing these studies into the University of Nebraska, and Dr. Baldwin has been called to represent them from his standpoint at the University of Toronto. Professor Ladd at Yale and Professor James at Harvard have both apparatus for demonstration and research in experimental psychology. In the faculty of Clark University Dr. F. Boas represents the psychological parts of anthropology, Dr. Donaldson neurology, including morbid psychology, and brain localization, Dr. Hall and Dr. Sanford experimental psychology, and Dr. Hodge, whose valuable work has been published in the JOURNAL, is a fellow. The *Anthropologist* and the *Folk-lore Journal*, both less than two years old, the establishment of an experimental station under the competent direction of Dr. Wm. Noyes at the McLean Asylum for the Insane at Somerville, and the valuable work of Dr. Cowles in interesting the American Association of Asylum Superintendents in experimental psychology, the work of the Seybert Commission, and of the American Society for Psychic Research, which is hardly less indebted than is this JOURNAL to the wealth and generosity of Mr. R. Pearsall Smith, the growing demand of our colleges for well trained psychologists, and, we may perhaps be permitted to add, the growing subscription list of this JOURNAL—these and many more indications promise new and better things in the study of the human faculties. In the number and scientific interest of its rare borderland neurological phenomena this country unhappily presents opportunities for certain lines of study unequalled elsewhere. Thus if we consider opportunity for observation, direct practical utility, the high degree of general interest and the wide inferences concerning the profoundest of all human interests which we expect eventually to see warranted in these fields, which we shall intimate later, we must conclude that we are now at the beginning of developments of great importance for science and for man.

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